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Epistemic Complementarity And Scientific Rationality

Arunasalam Balasubramaniam

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EPISTEMIC COMPLEMENTARITY
AND SCIENTIFIC RATIONALITY

by

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requirements for the degree of
Doctor of Philosophy

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ABSTRACT

Classical empiricism raised epistemological issues within a framework of dichotomies that were rarely questioned. It was assumed that statements were either normative or descriptive (and analytic or synthetic); terms were either observational or theoretical; meaning is either given atomically or holistically; truth was either radically independent of language (correspondence) or radically dependent upon it (coherence); and the entities postulated by scientific theories are constructed (instrumentalism) or are theory-independent (metaphysical realism). Modern philosophers have questioned the tenability of these distinctions and attempts have been made to relinquish many of them.

I argue that, instead of discarding them, we have to recognize these dichotomies as complementary aspects of epistemic notions, in the same way that the wave-particle dichotomy reflects complementary aspects of quanta. Epistemology has to be reconstructed upon a framework of new concepts that explicitly recognize epistemic complementarity. The basic unit of experience is a Gestalt (a theoretically structured pattern of given sense-data); statements are Expectations (normative and descriptive at the same time); terminological meaning is Holarchic (with atomistically given observational aspect, and holistically offered theoretical aspect); truth is Representation (which has a double dependence on language and the world); and the objects of science are Perspectival (being simultaneously constructed and discovered). Epistemic complementarity is ultimately traceable to the dependence of experience on both language and the world.

These notions enable us to construct a theory of scientific method and scientific rationality that resolves many crucial issues in contemporary philosophy of science. Radically different theories can be compared by appealing to a background language and experience constructed out of them (rather than a language independent of the competing theories); scientific revolutions can be recognized as possessing both cumulative and disjunctive features (which can be identified precisely through the background language); the internalist and externalist accounts of science can be seen as dual perspectives on the content of scientific theories (rather than incompatible alternatives); a theory of internal scientific rationality requires the deployment of dialectical methods (and not only the traditional deductive and inductive techniques).

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INTRODUCTION

The traditional epistemologies that were developed in the seventeenth century essentially adopted the view that all knowledge was erected upon an indubitably given foundation either in experience or in reason. For the empiricists like Locke and Berkeley the fundamental units of experience were the sense-data - colours, smells, tastes, shapes and so forth; for rationalists like Descartes they were self-evident truths that were given to the intuition. Such views are no longer compatible with modern science. That reason can furnish truths about the world that are independent of experience cannot be sustained by any appeal to the history of science. Even the transcendental categories of Kant have been found to be corrigible by the discoveries of modern physics and systems of geometry are recognised to be empirical theories that have to be tested against the world. That reason cannot furnish a foundational basis for knowledge does not mean that we can find one in experience. All that we perceive in the world is intimately affected by our theories about the world. There is no innocent eye that confronts reality: perception is structured by conception even if conception is supported by perception.

The notion of a foundational experience is contrary to many of the discoveries of contemporary science. Numerous studies in perception reveal that external stimuli do not completely determine what is experienced in awareness. Gestalt psychologists have unearthed a great deal of evidence to show that our experience of reality goes far beyond mere sensation. It is mediated by laws of organization of stimuli that

are an integral part of the structure of our sensory-brain system. It is also affected by knowledge we have acquired in the past as well as our expectations about the nature of what is being observed. Thus perceptual experience does not arise as a given that is solely dependent on external stimuli - it is also a construction that is constituted by gestalt laws of organization and acquired conceptual knowledge.¹

Developmental psychologists, like the structuralists of the school of Piaget, show how the child's experience of the world develops in well-defined stages along with the conceptual apparatus it deploys to deal with this experience.² They reveal that certain experiences would not arise, and can be checked from arising, if the conceptual apparatus is prevented from developing. Thus it becomes impossible to separate perceptual and conceptual development - the two are interdependent. This means that we cannot assume that we have an experience of the world that can be foundational and uncontaminated by the theories we have about the world.

The same result appears to be strongly indicated by findings in psychophysiology.³ These studies reveal that the sensory organs of all organisms are intimately linked in their structure and function to the role they play in their patterns of life. The sensory systems abstract, codify, select and transform the stimuli reaching them in a fashion designed to let through only what is of significance to the interests and survival of the organism. Human sensory organs do the same and it cannot be assumed that the external stimuli that give rise to experience are presented in an uncontaminated, uninterpreted and pure form. What arises in experience is interpreted information, and we have no grounds for supposing that this experience furnishes knowledge that

is any more foundational than that which we obtain from our theories about the world. This is to presume that the way our sensory systems process stimuli in presenting them to consciousness has some sort of epistemological priority over other forms of knowledge. The implicit theories built into our sensory organs are as corrigible as our explicitly held theories - and they often mislead us. They suggest that objects get smaller as they recede from us; that a stick partially immersed in water is bent; or that the moon follows us as we walk. To make experience foundational is to accord an incorrigible status to the way our sensory organs interpret information. Surely there are no grounds for doing this.

A great deal of research in linguistics reveals that language not only informs us about the world, but also influences our experience of this world. This thesis, known as the Sapir-Whorf hypothesis, is by now extremely familiar.⁴ It suggests that language does not merely describe what is given to experience; it also creates our experience to a considerable extent. The same sort of claim is made by cultural anthropologists. They are impressed by the variety of ways people living in different cultures experience the world and the potent force of language in shaping this experience. Thus what is experienced is determined, to a great extent, by the language through which this experience is acquired.

The view that experience provides foundational knowledge is extremely implausible if we take into account the theory of biological evolution. The kind of experiences we now have of the world evolved over millenia and are likely to evolve in the future. We do not experience the world in the same way as our ape-like ancestors did; even

less do we experience it in the same way as our aquatic or single-celled ancestors. It is also extremely unlikely that our descendents a few millenia hence will experience the world the way we do now. As our brain and sensory organs evolved so did our experience. What makes the experience we are capable of having now so special that it can furnish incorrigible knowledge? It is, of course, reasonable to require that we have to refer to experience to obtain knowledge about the world, but it is unreasonable to presume that this experience provides a foundational basis for knowledge.

Even the history of science reveals how misleading it is to suppose that experience can lead directly to foundational knowledge. What could be more evident to the senses than that the earth is stationary and that the sun, moon and stars move in the heavens? Yet Galileo commended Copernicus' courage in performing this 'rape of the senses'.⁵ What could be more clearly given to experience than that space and time are separate and radically different entities? Yet Einstein revealed how what is spatial to one person can appear temporal to another. How can such knowledge be considered acceptable if we make experience foundational? Knowledge can be supported by an appeal to experience - and has to be so supported - but this experience cannot be deemed to be an incorrigible basis for knowledge.

It should be evident by now that the notion of a foundational experience cannot be reconciled with contemporary science. However, when we attempt to reconstruct epistemology on a nonfoundational basis we confront tremendous problems. For by making experience depend on language we also appear to relativise to the conceptual framework many of the fundamental notions of epistemology. Our observation reports,

the meaning of our terms, the empirical support of theories, truth and scientific progress all seem to become relativised. Thus there appears to be no overarching court of appeal in which conflicting conceptual frameworks can be evaluated. Within a foundationalist view scientific objectivity was deemed to be achieved by checking observation reports, giving terms meaning, testing the empirical support of theories and defining truth and progress by appealing to an independently given experiential basis.

Cultural relativism has been embraced by many sociologists and anthropologists. They argue that for widely separated cultures the linguistic, social and historical differences lead to such radically different conceptions of the world that it is impossible to determine which of them is objectively preferable. We cannot attempt to decide which culture provides the best understanding of the world because there are no standards over and above those provided by a specific culture. We have to comprehend and evaluate cultures from within their specific social and historical contexts and, as far as possible, from within their linguistic and conceptual frameworks. Cultures cannot be understood or evaluated outside their specific occurrences and by means of standards that transcend what each of them offers. They are conceptually incommensurable worlds.

This relativistic view has been extended by some philosophers into epistemology so as to deal with the problems presented by nonfoundationalism.⁶ They assume that language so infects our experience of the world that we cannot have any objective means of comparing world-conceptions. Every world-view can only be judged from within the framework it provides; we cannot objectively compare different world-views. They

argue that theories like Newton's theory of gravity and Einstein's theory of relativity, or the theories of Ptolemaic or Copernican astronomy are so comprehensive that they can be considered to be alternative world-conceptions. The experience supporting each of them is determined by the framework offered by the theory. Observation reports and terminological meaning are intimately affected by the theories themselves. Hence these theories cannot be objectively compared - they are incommensurable world-views.

However, even if these holistic views provide nourishing food for cultural anthropology they can only furnish unpalatable fare for epistemology. When anthropologists examine a culture other than their own it is a healthy hermeneutic principle to purge themselves of their own cultural orientation and attempt to understand the other culture from within its own framework. Their concern is not to evaluate this culture but to comprehend it. In the same way intellectual historians (including historians of science) must attempt to understand the conceptual frameworks they examine from the perspective of the historical actors. However, epistemology is not concerned only with understanding different conceptual frameworks but also with evaluating them. Evaluating involves understanding but evaluating is not just understanding. Thus in epistemology we cannot be satisfied by the relativism involved in making different conceptual frameworks incommensurable.

The epistemological relativism of holistic empiricism does not take seriously the radical changes that are required in adopting a nonfoundationalist view of experience. It assumes that by relativising all knowledge to a conceptual framework the problems of epistemology become resolved. Furthermore, it does not deal with the problem of comparing

frameworks except by weak appeals to nonrational or sociocultural factors. It is actually a form of foundationalism within a framework. It assumes that given a framework there is a foundational experience. Thus within the framework we can deal with observation, meaning, truth and progress in the traditional way. Hence the inherited tradition of epistemology is accepted in toto except that it is now relativised to a particular framework. Comparisons between frameworks, however, are impossible because they are incommensurable.

The view that I am proposing avoids the Scylla of foundational absolutism and the Charybdis of nonfoundational relativism. The former makes sense-data the basic units of experience; the latter denies that there is anything in experience that is independent of language or theoretical presuppositions. If we refer to the former as atomic empiricism we can assume that it views experience as given in atomic units of sense-data which provide the foundation upon which conceptual knowledge is supported. Since these are given independently of conception there is an ultimate court of appeal to test all knowledge. The holistic empiricists deny that there are any sense-data - every part of experience is affected by its conceptual context. Thus there is no court of appeal independent of a particular linguistic framework.

I shall argue that though there are sense-data independent of theories it is not these that are the basic units of experience. Perception involves the apprehension of gestalts which are structured patterns of given sense-data. Our language determines the way we organise sense-data into perceptual units, but this does not mean that there is no sensing of sense-data independent of theoretical presuppositions. Thus the fundamental unit of experience has a part that is theoretical - the way

sensations are organized - and a part that is independent of theoretical contamination - the sensations that arise in experience. To distinguish this view from both atomic empiricist foundationalism and holistic empiricist nonfoundationalism I shall refer to it as gestalt empiricist (non)foundationalism.

Gestalt empiricism requires us to relinquish some of the most fundamental presuppositions of classical theories of knowledge. We can no longer assume that a separation can be made between normative and descriptive or between theoretical and observational statements. Statements exhibit a dual aspect that allows us to treat them as both normative and descriptive. Furthermore, though a distinction can be made between statements that are reports of observation and those which are not we have to deem all statements as essentially theoretical in nature.

This also requires that we cannot make the traditional distinctions between a priori and a posteriori, conventional and empirical, or analytic and synthetic statements. Neither can we assume the holistic empiricist view of Quine⁷ that we ought to discard the attempt to make such distinctions. The distinctions are tenable but they can no longer be seen as distinctions between dual classes of statements, but as dual ways of construing the same statement. Many of the problems confronting epistemology today can be traced to the futile attempt to make these distinctions between statements in a significant way or to do away with the distinction altogether. Similar considerations would also lead us to recognise that we cannot separate methodological and ontological statements. All methodological statements are as much ontological as ontological ones are methodological.

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Traditional theories of meaning also have to be discarded. Such theories assumed that meaning could be given independently of empirical claims and that an important distinction could be made between theoretical and observational terms. We shall see that all terms can possess an observational and a theoretical component of meaning, but that this is not grounds for making a distinction between terms. Neither an atomic theory of meaning suggested by traditional atomic empiricist views nor the holistic theory of meaning proposed by others in recent years can do full justice to the way that scientific terms are given meaning. Instead I propose a theory of meaning that is holarchic - one which recognizes that terminological meaning has both an atomic and a holistic component.

The problems connected with the way terms change their meaning when embedded in different theories has led some holistic philosophers to argue that such theories are incommensurable and cannot be compared. Unsuccessful attempts have been made to deal with this problem by creating either a pure observation language or a neutral background language that is independent of the theories being compared.⁸ The problem is aggravated by the fact that there is also no independent empirical basis to compare the theories since all perception is theory-laden. We shall see that comparison between theories can be effected by constructing a language out of the very theories in conflict. This does not require us to appeal either to a pure theory-independent language or a language independent of the theories in competition. We use the frames in conflict to construct a language of mediation - what I have called a background equivocating language - and an acceptable empirical basis to test them against one another. Thus the testing of competing

theories is not achieved by appealing to a language and experience that transcends either of them but by creating a language and experience out of both of them. This approach will also reveal the limitations of deductive and inductive reasoning to test and compare theories - we have to invoke dialectical patterns of reasoning that include, and transcend, inductive-deductive reasoning.

The problem of the rational evaluation and comparison of scientific theories is central to any epistemology. Traditionally it has been supposed that the important values involved in theory comparison are simplicity, explanatory and predictive power, and generality. Simplicity, however, is not a test of the empirical adequacy of a theory: it merely measures its conceptual economy. We shall see that the emphasis on simplicity involves more than a desire for conceptual economy - simple theories are precisely the ones that offer the greatest possibility of serving as theories to structure perception. If all experience is theoretically structured it is the simple theories that are capable of doing this most adequately - i.e., they are the ones that can not only serve as explanatory systems but also as observational instruments. Furthermore, we shall find, contrary to all traditional views, that values like explanatory and predictive power or generality are not crucial to the epistemological evaluation and comparison of theories. The important value involved in admitting, accepting and rejecting theories is what I have called its relative explanatory indispensability. The reason that the traditional values have been deemed crucial is that the measure as given by the relative explanatory indispensability often coincides with the measures provided by explanatory power, predictive power or generality. However, there

are many examples in the history of science which show that where there is a conflict it is the relative explanatory indispensability that is crucial to determining a theory's admissibility or acceptability.

The dual aspect inherent in statements and the dual components of terminological meaning also requires us to adopt a theory of truth that reflects this duality. Of the two great classical theories of truth the correspondence theory is often linked to an atomic conception of meaning and is generally espoused by atomic empiricists - e.g. Russell and the early Wittgenstein; the coherence theory is usually proposed by those who also hold a holistic theory of meaning - e.g. Quine. It is not surprising, therefore, to find that neither of these theories is adequate once we recognize that terms have both an atomic and a holistic component of meaning. What we need is a representation theory of truth. Such a theory cannot be construed as variant of the correspondence theory as is usually done: it is actually an alternative to these two classical theories.

The representation theory allows us to recognize that the metaphysical realist assumption that there can be only one true theory is no longer tenable. This suggests that we have to reject the realist view of the objects offered by true theories without adopting the constructivist views of holistic empiricists like Kuhn or Goodman. What we need is a perspectival view that enables us to affirm that there is a world that transcends our theories but that what is conceived and perceived to exist in this world is theory-dependent. There is no single true perspective on the world even if truth is an idealized acceptable representation of it. The objects perceived and conceived to be in the world are both constructed and discovered; invented and found.

The duality involved in statements, meaning and truth is also reflected in what we say exists in the world. This duality demands a perspectival rather than a realist or a constructivist account of what there is.

The perspectival view of objects and the representation theory of truth allows us to acknowledge that the content of theories is underdetermined by the requirement that they be true. This enables us to integrate what are often considered to be incompatible approaches in epistemology, i.e., a theory of rational evaluation that allows an internalist account of science with a sociocultural externalist account that seeks to rationally explain the content of accepted scientific theories. These are two different but compatible perspectives on science - an external perspective that rationally explains the content of scientific theories and an internal one that rationally evaluates their content. These dual perspectives replace the inadequate distinction traditionally espoused by separating a context of discovery from that of justification.

Traditional epistemologies generally attempt to restrict or deny any role for metaphysics in science. The view developed here reveals the seminal role played by metaphysical paradigms in the history of science - they are the seeds from which scientific theories sprout. They are also crucial to understanding how external influences become constitutive of the content of specific scientific theories. This importance of metaphysics, nevertheless, does not require us to conflate metaphysical with scientific theories.

Atomic empiricists view the growth of science as cumulative. Though they recognize the role of the revolutions introduced by Einstein or Bohr, they see these as adding to the advances already made by

classical physics. In more recent years holistic empiricists like Kuhn and Feyerabend have emphasized the disjunctive nature of these revolutions - they view them as bringing about such a radical revision of experience and conception that the post and pre-revolutionary sciences cannot be compared. Those who perceive revolutions as disjunctive see change without progress; those who hold them to be cumulative see progress without change. We shall find that revolutions are neither cumulative nor disjunctive - they are metamorphic. Such revolutions can be shown to involve both change and progress. Also, it is the very same processes that bring about the growth of science between revolutions that trigger and effect metamorphic revolutions. Given the representation theory of truth such revolutions can be directly interpreted as involving progress towards the truth. Thus an account of the noncumulative growth of science is possible without giving up either the notion of truth or that of progress.

The rest of this work will be an attempt to elaborate the gestalt empiricist view as a more adequate alternative for epistemology than atomic empiricism or the recent variants of holistic empiricism. As I have indicated this requires us to give up the basic dualisms built into traditional epistemic categories. We cannot classify statements into prescriptive and descriptive; analytic and synthetic; or theoretical and non-theoretical. Terms cannot be divided into observational and theoretical; nor can meaning be interpreted through an atomic or holistic theory. Truth is neither coherence nor correspondence and the objects postulated by true scientific theories are not interpretable in a realist or constructivist manner. These dualisms are not given up in order to embrace a monism that refuses to recognize the distinctions

mentioned, or attempts a reduction to one or the other of them. The distinctions are recognized but not as being exclusive. They are seen to reflect a double aspect - or complementary aspects - of that to which they apply. Statements have the complementary aspects of being prescriptive and descriptive; analytic and synthetic; a priori and a posteriori. Terms have a double aspect of observational and theoretical meaning; this in turn gives their meaning an atomic and a holistic dimension. Truth depends both on a relation to the world and to the other beliefs we have about the world. What we recognize to exist in the world depends both upon the theories we accept as true about the world and the nature of the world that we confront in constructing theories.

Atomic empiricists tend to overemphasize the nonlinguistic aspects of experience - hence their attempt to recognize only sense-data as units of experience. Holistic empiricists exaggerate the linguistic construction of experience - hence their efforts to deny any role for given sense-data. The former affirm experience by denying any role for language in constituting experience; the latter make language constitutive of everything given to experience. Gestalt empiricism recognizes that experience is structured sense-data - or gestalts. Gestalts have a double dependence on language and the world. Experience is structured by theories that are themselves supported by an appeal to experience. This interdependence of experience and language requires us to recognize complementary aspects in every important epistemological category - statements, meaning, truth and the objects postulated to exist by our true theories.

The reader may have recognized that the dual aspect of epistemic categories I am proposing bears an analogy to the dual aspect of matter recognized by physicists. Classical physics divided material events into those that exhibited particle-like and wave-like properties only to recognize subsequently that all matter had both corpuscular and wave properties. The quantum view is not an attempt to merely synthesize by addition the earlier views or to tread a middle ground; nor does it enable us to reduce physical properties into either only particulate or wave-like ones. It is a radically different alternative that excludes both the denial of the wave-particle distinction and the use of the distinction to divide matter into different classes. In the same way gestalt empiricism is not an attempt to merely synthesize atomic and holistic empiricist views, or to tread a middle ground. It does not allow us to reduce philosophical properties into one or another of the dual aspects involved. It requires us to view the complementary features of statements, meaning, truth and objects as an inherent, ineradicable and irreducible feature of their very nature.

CHAPTER ONE

THEORY AND OBSERVATION

1.1 Introduction

The cornerstone of classical empiricism was the view that all knowledge was erected upon an independently given basis in experience. For positivists the experiential foundation was provided by the sense-data; for Kantians the experience itself was constituted by the categories but since these were transcendental and operate in the same manner for everyone there was no denial of a foundational basis in experience. These traditional empiricists generally assumed an atomic view of experience - what was given in experience were units of sense-data¹ or physical objects. This view has now come under sustained attack by those who espouse a holistic view of experience. Philosophers like Kuhn and Feyerabend maintain that there are no foundational givens in experience - all experience being theory-laden our conceptual framework as a whole determines our experience as a whole, but no aspect of experience can be taken to be a given. Even sense-data are rejected by them as givens; or as constituting a basis for theoretical knowledge.

Classical empiricists also subscribed to an atomic theory of meaning. They assumed that what was given in experience could be represented in a theory-free language. Such a language was instituted by the establishment of linguistic conventions that linked the terms of the language individually to elements given directly in experience. Though there were problems connected with this program, it was felt that this theory-free observation language provided the medium in which all observation reports could be formulated and in which all theoretical

knowledge could be tested.

The holistic empiricists deny that language can be instituted in this atomistic manner. Since there is no foundational experience independent of all theories for them they affirm that linguistic conventions cannot be instituted by referring to an experience independent of all presuppositions. Even in teaching the use of terms to represent events in experience we have to appeal to an experience constituted by our theories. The denial of a foundational experience leads them to deny any theory-free observation language. The theory-ladenness of experience means that all terms have a meaning defined by the theoretical framework in which they occur and all observation reports are theory-laden.

Neither atomic nor holistic empiricism can do justice to what is given to perception. Perception involves the apprehension of gestalts which have both given and nonfoundational aspects. The collapse of a foundational observation language requires us to view the empirical statements of science in a new way. Such statements cannot be construed merely as descriptions about events in the world; they have also to be considered as demands on our part as to how events in the world should be construed. These complementary aspects of statements require us to deem them as being both claims about how events in the world can be represented, and as demands on the way we should represent events in the world. Statements that exhibit this dual aspect I call expectations.

I propose to show that the expectational view of statements requires us to relinquish any attempt to make a distinction between observational and theoretical statements. All observation reports are also theoretical. This still allows us to maintain a distinction

between statements that are reports of experience and those which are nonreportive. But reportive and nonreportive statements are both theoretical. We also have to give up the distinction between conventional and empirical, or analytic and synthetic statements. The classification of statements into these dual classes in traditional philosophies represents an attempt to deal with dichotomies that do not belong to separate classes of statements, but reflect the complementary aspects of every empirical statement. As physics divided matter into two classes that exhibited wave-like and particle-like properties before realizing that these properties belonged to all matter, so in philosophy we have divided statements into separate classes on the basis of properties that are shared by all statements. Before we examine the arguments that lead us to this view let us consider the thesis that all observations are theory-laden.

1.2 The Theory-ladenness of Observation

The figures used by gestalt psychologists furnish classic demonstrations of the theoretical nature of all perception. They reveal that what is perceived in experience goes beyond the sensory stimulus presented to the observer. Figures such as the duck/rabbit and faces/vase can be experienced in more than one way though the image on the retina is held unchanged. In the case of the duck/rabbit we confront the same figure but depending on the context we may experience seeing it as a rabbit or a duck. Similarly the faces/vase figure can be seen as a drawing of two profiles confronting each other or as a white urn bounded by a black background. Numerous examples of this sort show that experience involves far more than the mere reception of external

stimuli - it involves organizing such stimuli into a coherent pattern. It is the organized gestalt that is perceived. The same observer confronting the same stimulus may see different things when he organizes the stimulus in different ways.²

Clearly to experience something as a rabbit requires prior knowledge regarding the nature of such creatures. Someone acquainted with ducks but not rabbits cannot be expected to undergo the same sort of mutability of experience that we do. Such a person can experience seeing the duck but under no circumstances could we suppose that he would perceive the rabbit. What is experienced depends a great deal on the sort of collateral information we bring along prior to that experience. Perception requires more than the sensing of stimuli - it is the dynamic structuring of sensory evidence into meaningful objects and how we do this depends on the background knowledge at our disposal.

The examples offered by ambiguous figures like these have been used by many philosophers to support the holistic empiricist view that all perception is theory-laden and, consequently, we have no foundational experience for knowledge. The nonfoundationalists argue that since experience itself is conceptually tinted there is no neutral ground for the empirical testing of theories. In fact many of them go so far as to say that there is no given in experience - even the sense-data of the phenomenologists are rejected by them as either irrelevant or nonexistent.³

Such claims appear convincing only because they embody a partial truth that corrects the misconceptions of phenomenalist predecessors who held scientific knowledge to be either totally founded on sense-data or to involve merely the conceptual accounting of sense-data. The non-

foundationalists rightfully point out that perception goes beyond sensation; they are mistaken in claiming that sensations do not furnish a given in experience. In the example of the duck/rabbit it is possible to recognize that one figure is involved only because both experiences are seen to arise from the same sense-data -- a particular structure of lines. What is given to the visual field as sense-data is distinct from what is perceived in the field as a result of organizing this data into a patterned whole. The gestalt experience arises from the interpretation of the sense-data but this in no way pre-empts us from being able to distinguish one from the other.⁴

The important feature identified by the nonfoundationalists is that what is observed has to be differentiated from what is given as sensation. We observe a rabbit; not a particular configuration of lines. We report seeing a rabbit; not pure sense data. This means that the observation report is a response to a structured field of sensation -- a gestalt. This gestalt has given and theory-laden aspects: it is a network of conceptually determined relations seen in a theory-independent complex of sensations. Since the experience is of a gestalt we have to reject the view that experience is of foundational sense-data; but we also have to deny that experience has no element that is given. The gestalt offered to experience is foundational in respect of the sense-data it contains, and nonfoundational because of the theoretical relations utilized to organize the sensation-field. The experienced gestalt is more than an atomic array of given sensations; it is less than totally holistic and nonfoundational.

I want to say that the gestalt is the fundamental unit of all experience. Thus I reject the atomic empiricist view that the basic unit

of experience is sense-data; nor do I wish to espouse the totally holistic view that the basic unit of experience contains nothing that is given. The unit of gestalt contains a conceptually tinted structuring of given sense-data. For this reason we cannot describe it as totally foundational or totally nonfoundational. It is (non)foundational. Such a view can be said to be nonfoundational provided it is recognized that unlike the holistic empiricists I allow that sense-data are given even if they are not the foundational units of experience.

Though the gestalt is a fundamental unit of experience, it is possible for one gestalt to be constituted by a number of other gestalt elements. The perception of the larger gestalt may be mediated by the perception of its component gestalts. This is not obvious in the simple examples cited where the gestalt experience is achieved quite spontaneously. Take the figure of the old/young woman.⁵ Some people may find it easy to see the young woman but try as they might they cannot see the old woman. We have to guide them towards the experience by delineating various elements of the new gestalt. These elements of the gestalt are not sense-data - they are themselves gestalt units. We do this by means of statements like "Can you see her nose? These are her eyes. Notice the protruding chin. This little wedge is her mouth." and so on. It is through such a process of interpretation that these persons finally recognize the image of the old woman. The image arises suddenly and apparently spontaneously but not without an interpretive context in which it can occur.⁶

What is significant about the process described is that though the interpretation requires cognizing sense-data it is neither mediated by appeal to sense-data nor is it about sense-data. The interpretation

uses sense-data as indicators⁷ for elementary gestalts - 'nose', 'eye', 'chin' and 'mouth'. These elementary gestalts are themselves components of a larger gestalt. Thus seeing the figure of the old woman in the figure previously seen as a young woman requires establishing a new field of relations on given sense-data. A perceived image is constructed out of sensory indicators. By themselves sensations are a meaningless Rorschach - seen through an interpretation they reveal meaningful entities.

Is this not what happens in all perception? Looking at the world we do not merely perceive sense-data. Beyond the shifting pattern of sensation we see a world of (semi-)stable objects - trees, clouds, sun, stars. It is true that without sense-data we could not see them; but neither would we see them if all that we saw were sense-data.

Sense-data are indicators of external objects but the perception of objects goes beyond sensation. Perception involves the active imposition of order on such data through a multifarious network of interpretations both acquired and inherited. All objects seen in the world are gestalts constructed by interpreting the sensational indicators they offer to experience. Not only that but each perceived gestalt may itself be seen to be made of other perceived gestalts - forests contain trees; trees are made of branches; branches carry leaves. Rocks make up mountains and stars group into constellations. People divide into organs and grow into crowds. Without such an organization in awareness of objects into gestalts, made up of and making up other gestalts, it is difficult to see how we could conceptually handle the information furnished by the external world.

The gestalt nature of all experience makes evident the theoretical nature of all perception. What is true of everyday life is even more manifest in the case of scientific observation. The oft-held view that scientific objectivity involves confronting the world without any presuppositions grossly misconstrues the nature of observation in science. One of the earliest to recognize this was Duhem:

Enter a laboratory; approach the table crowded with an assortment of apparatus, an electric cell, silk covered copper wire, small cups of mercury, spools of wire, a mirror mounted on an iron bar; the experimenter is inserting into small openings the metal ends of ebony headed pins; the iron oscillates, and the mirror attached to it throws a luminous band upon a celluloid scale; the forward-backward motion of this luminous spot enables the physicist to observe the minute oscillations of an iron bar. But ask him what he is doing. Will he answer "I am studying the oscillations of an iron bar which carries a mirror"? No, he will answer that he is measuring the electrical resistance of the spools. If you are astonished, if you ask him what his words mean, what relation they have to the phenomena he has been observing and which you have noted at the same time as he, he will answer that your question requires a long explanation and that you should take a long course in electricity.

Duhem is pointing to an aspect of science that should be evident to anyone not blinded by the dogma that all we actually experience are the so-called phenomena (or sense-data). What the physicist observes are electrical batteries, bar magnets, calibrated scales, conductors, insulators and resistors. To the uninitiated experiencing such objects would be impossible however carefully they look. This experience can only be obtained after one has learned to look at the world through the physicist's eye - or rather through a particular network of physical theories. It is true that any layman would confront the same sense-data as a physicist but it is inconceivable that someone unacquainted with electrical theory could experience the gestalt objects the scientist perceives.

Hanson has used similar arguments to suggest that all scientific observation is theory-laden. He considers the case of Kepler and Brahe looking at the sun at dawn.⁹ For Kepler the movement of the sun against the horizon is due to the spinning of the earth. He experiences the horizon as sinking relative to the sun. For Brahe this same movement is due to the sun orbiting the earth. Thus to him the sun would appear to rise above the horizon. Though, in one sense, they both confront the same event, they nevertheless experience it differently because they see it through different theories.

It may be said that we need not accept this account. One may suppose that they both experience the sun moving relative to the horizon. Brahe interprets this movement as due to a geocentric orbit performed by the sun, and Kepler views it to be the result of the daily rotation of the earth. However, is this relative motion of the sun and horizon really what is experienced? It is certainly an interpretation of what they experienced in a language to which both of them would agree.⁸ Are we to suppose that all those in the past who saw the earth as stationary and perceived the sun move relative to the horizon actually experienced a relative motion, and only interpreted this motion as a rising sun?

Can this formulation of the experience in terms of relative motion deal with that ancient Greek theory where the sun and the stars were considered to be glimpses of the celestial fires obtained through pores in the rotating heavenly vault? For if this theory were true what an astronomer would see at dawn would not be the movement of a sun, but only of a hole in the heavens. There would be no bright yellow object actually moving relative to the horizon. Clearly it would be possible

to formulate the event observed in a new locution such that Brahe, Kepler and an ancient Greek astronomer would agree that it refers to what they observe. But my point is that it would no longer be an account of their experience, but an account based on their experience to which all of them could agree. All of them could agree that they see a circular yellow image move relative to the horizon. But none of them need experience this. Brahe experiences a rising sun; Kepler perceives the horizon sinking away from the sun; and the ancient Greek astronomer sees different portions of the celestial fires as the celestial vault turns. Responding to the same sensory stimulation each experiences a gestalt constituted by his specific theory.

The gestalt (non)foundationalist view of experience allows us to circumvent the twin pitfalls offered by atomic empiricist foundationalism and holistic empiricist nonfoundationalism. Those atomic empiricist with phenomenalist inclinations want to make atomic sense-data the foundational units of experience. Such a view tends towards idealism and is extremely difficult to reconcile with scientific realism. If all that is offered to experience are sense-data, and knowledge is founded on experience, it is difficult to see how one can assume a world independent of the phenomena. Furthermore, as we have seen, such an atomic empiricism is incompatible with experiments that suggest that experience involves more than the reception of sensory stimuli.

In contrast the holistic empiricists generally tend to espouse scientific realism. However, the holism of Hanson, Kuhn and Feyerabend leads them to reject the notion of any given in experience. In particular they deny that there is any role for given sense-data.¹⁰ However, a closer scrutiny of their arguments shows that they furnish no

reason for supposing that any experienced sensation - as distinct from an organized pattern of sensations - is crucially affected by our theoretical presuppositions. Though their arguments appropriately reveal the theoretical determination of experience they do not impugn the view that sensations can be considered, in some manner, as given.

Take Kuhn's objections to the notion of the givenness of sensations.¹¹ Firstly, he points out that the traditional attempts to found science upon given sense-data have failed because no attempt to provide a sense-datum (or phenomenalist) language for science has been successful. Secondly, there is a great deal of neural processing that is involved before an external stimulus is experienced as a sensation: then there is no direct theory-free correlation between an event in the world and the sensations it gives rise to. Furthermore, there is no one-to-one correlation between stimulus and sensation since an infinite number of combined wavelengths of light can give rise to the same color sensation. Finally, our responses to stimuli cannot be innate since we can learn to discriminate colors that we may have found indistinguishable prior to training. This leads him to conclude that "though (sense) data are the minimal elements of our individual experience, they need be shared responses to a given stimulus only within the membership of a relatively homogenous community, educational scientific or linguistic".

Kuhn's extreme conclusion is unwarranted. For one can accept sensations as given and yet deny that a phenomenalist language of science is possible. This would be the case if we adopt the gestalt view of experience. Then sensations would be merely indicators of external events, but science need not be about sensations. We read sensations in order to obtain knowledge about our external world, but

the content of what we read does not depend only upon the sensations we confront. It is also affected by the theories we already hold about the world. Neither can it be an objection to the gestalt view that a great deal of neural processing occurs between a stimulus and a sensation; or, that there is a many-to-one relation between impinging stimuli and sensation. This would be problematic only on the view that our knowledge is about sense-data; or that sense-data furnish theoretically uncontaminated information about the world. It is no objection to the view that sensations are indicators that can be read to obtain information about the events to which they are causally connected.

Furthermore, we cannot presume that sense-data are not given because learning is sometimes necessary to discriminate between colors previously not distinguished. This could be a consequence of heightening our sensitivity to differences already present in the sensations given to experience. Even where we do learn to distinguish between colors that we originally did not - between marine blue and sky blue, say - it is doubtful if one can claim that the difference was not originally present in our experience. Kuhn's account seems to suggest that training actually creates the difference when it may only be heightening our ability to recognize the difference. An untrained subject may not be able to differentiate marine and sky blue when shown them in succession but it is unlikely that he will not notice a difference if they are shown him simultaneously. There is no evidence that someone can be trained to see a distinction between colors that is not seen when these are placed adjacent to one another prior to any training. Should this be possible then many of the experimental results obtained by colorimetrists in measuring the sensitivity of the human eye

to color differences (i.e., results that give the just noticeable differences between various hues) are subject to a fatal unacknowledged flaw: the just noticeable difference can be altered by training. There is no experimental evidence that this actually occurs.

What evidence there is tends to suggest the contrary. Subjects may appear to be able to discriminate colors on the basis of their verbal responses and yet turn out not to be able to distinguish them as sensations. Thus those who are color blind to red-green may report grass to be green and blood to be red though a more exacting test may show them unable to separate these colors as sensations. Thus even though they share responses to a given stimulus as members of a linguistic community - and these are learnt responses - they cannot be said to be able to experientially distinguish the colors in the way most people do. Kuhn's analysis appears to ignore this crucial point.

My argument for the givenness of sense-data, however, should not be taken as an argument for their foundational status. In the first place, as I have already argued, observation involves the perception of gestalts - even if these are organized around sense-data. Secondly, and more important, theory is also required to separate what are relevant sense-data from spurious ones in order to determine which of these are to be taken as indicators of the external events we are concerned with, and which are merely observations induced by our sensory-brain systems or events irrelevant to those we are examining. This arises even when we are employing scientific instruments.

For example, Feyerabend has shown that, when the telescope was first used as an instrument of astronomy by Galileo, it was difficult to separate the properties of the objects seen through it from the

'illusions' created by the instrument. The instrument distorted images, discoloured objects, and even introduced coloured fringes due to chromatic aberration. There was no way of deciding, merely by appealing to telescopic experience, what were heavenly phenomena and heavenly illusions. This ambiguity of the experience offered by the telescope led to heated controversy in the seventeenth century - it was not merely dogmatic conservatism that led many esteemed astronomers to reject Galileo's findings through the telescope. Only the development of an adequate theory of the telescope - an achievement of Kepler's - ultimately allowed scientists to separate the relevant from the spurious phenomena.¹²

This has consequences for any theory that invokes sense-data as given indicators of events in the world. It requires us to suppose that, even if the relevant sense-data are causally connected to external events, we still need to appeal to theoretical knowledge in order to separate the relevant from the spurious sense-data. Sense-data may be given in a theoretically uncontaminated form, but they cannot be made a foundational basis for knowledge. The givenness of sense-data does not guarantee that they are foundational. Thus Galileo may have been responding to given sense-data but, until he could provide a theory to separate the relevant from the spurious data, his opponents were correct in withholding assent to his observation reports.

The distinction between the given and the foundational status of sense-data is often not distinguished in the debates between the atomic and holistic empiricists. It is assumed that showing sense-data are not foundational is equivalent to proving that they are not given. This is a fallacious argument. For to show them to be nonfoundational is merely

to reveal that any statement that arises, in their context already presupposes other theoretical claims; it is not to show that sense-data can be affected by theoretical beliefs. Thus there could be given sense-data which are, nevertheless, not foundational for knowledge claims.

The atomic empiricists have generally held that sense-data are given, and that they constituted the foundation upon which knowledge was erected. Holistic empiricists, like Kuhn, hold that there are no given sense-data and that, consequently, there is no foundational basis for knowledge. I want to suggest that sense-data are given, but that they do not furnish a foundational basis for knowledge. Any observation report is made in the context of theories that, not only allow us to distinguish spurious from relevant sense-data, but also constitute the gestalts to which such reports appeal. Thus gestalt (non)foundationalism requires us to assume that sense-data are both given and nonfoundational.

The view that there are no given sense-data is most closely associated with Sellars. He argues against them on two grounds. Firstly, he claims that our scientific accounts of the world ought to take precedence over the common sense ontology we have inherited. Sense-data - like, e.g., colours - do not have a place in our scientific framework. Hence, speaking scientifically, they do not exist.¹³ This, essentially, is a reductionist attempt that recognizes as scientific, only theories of physics and chemistry. Psychoanalysis and colorimetry, for example, certainly appeal to pains and colours. Unless one wants to exclude these as scientific disciplines the argument will not go through.

His stronger argument against sense-data, however, is based upon the claim that there is nothing that can be known prior to, and independently of, language.

"all awareness of sorts, resemblances, facts etc., in short all awareness of abstract entities - indeed, all awareness even of particulars - is a linguistic affair. ... not even awareness of such sorts, resemblances and facts as pertains to so-called immediate experience is presupposed by the process of acquiring the use of language".¹⁴

To support his case he makes the distinction between awareness as discriminative behaviour and awareness as being "in the logical space of reasons, of justifying and being able to justify what one says." Rats, amoebas and computers exhibit the first sort of awareness; awareness in the second sense is only manifested by beings who can utter sentences that are intended to justify the utterance of other sentences. Thus he wants to distinguish awareness as ability to respond to stimuli from awareness as justified true belief - or knowledge. Discriminative ability furnishes the causal conditions for knowledge, but does not provide the grounds for knowledge. Even the knowledge of particulars like "This is red" is made in the context of other propositions.

It seems to me that Sellars has only shown that any propositional assertion always presupposes other propositions. If knowledge is of propositions then there are no given or foundational propositions. However, this does not reveal that there is no given in awareness. Sellars distinction between the two sorts of awareness is crucial to make this point. Even if there is nothing that is given or foundational in the second sort of awareness, there may be given in the first sort of awareness. This is precisely what I want to say. Also knowledge of the external world requires, even if it is not grounded upon, awareness of

the first sense. Without such discriminative awareness it is impossible to understand why knowledge could be said to be about a world that is independent of propositions. To demolish the myth of the given in the second sort of awareness does not reveal that the given is a myth in the first sort of awareness.

The excessive holism of Kuhn's and Sellars' views and the radical dependence of the experience they invoke on theory - and this is a consequence of their denial of given sense-data - makes it extremely difficult for them to explicate the connection between theory and the world. In fact writers like Kuhn and Feyerabend appear to have relinquished any attempt to do so. They argue that since different theories give rise to different experiences they offer incommensurable worlds.¹⁵

Gestalt (non)foundationalism allows us to recognize that there is more to experience than sense-data. It endorses the theoretical nature of perception emphasized by the holistic empiricists. Nevertheless, it does not undercut the connection with the world because it accepts the givenness of sense-data even if these are not deemed foundational. Confronting the same sense-data different theories may structure them differently. This does not mean that the identity of the events they are about cannot be recognized, for it is precisely the sense-data that serve as indicators of external events. Thus an empiricism founded on the notion of gestalt elements in experience - gestalt empiricism - allows us to avoid the break with the world of events that holistic empiricism threatens (its realism notwithstanding) and that atomic empiricism embraces (with its phenomenalist learnings).

1.3 Observation Reports as Expectations

The gestalt view of experience requires us to radically revise the traditional notion of observation statements. This view holds that observation reports are archetypal examples of descriptive statements. Such a view can no longer be sustained. Our experience of the world comes to us through our theories, and by choosing to examine the world through different theories we can experience the world in different ways. This means that even our most basic observation reports about events in the world are founded on experience that is itself constituted partly by the theories through which we look at the world. This requires us to conceive of observation reports not merely as descriptive, but also in some sense as prescriptive of how we should represent the world.

Take an event that is given to us in experience. Our experience of this event is in part determined by the observational theories through which we observe the event, but it is not totally determined by this conceptual framework because it is also causally connected to the external event. The experience that arises is therefore a product of our observational theories and the external event. However we cannot suppose that we can separate the experience into two components - one given by the conceptual framework and the other contributed by the external event. The event - for example, sunrise - is a complex gestalt of patterned and dynamically changing sense-data. Consider the observation report that is now formulated on the basis of this experience. What is the relationship between this report and the external event? We have seen that interpretive observational theories are crucially involved in determining the nature of the report. For

example, deciding whether to report that the sun rises over the horizon or the horizon falls away from the sun not only depends on whether one experiences certain sense-data causally linked to an external event but also upon whether one assumes the earth to be stationary or in motion. Thus, the report is a claim that the external event can be represented in a particular way and a demand that it be so represented.

What then is the report? Let us say that it is simultaneously a demand on how the external event should be represented and a claim that the event can be represented in a certain way. The nature of the report depends both on the demands imposed by the observational theories and the constraints imposed by the external event. This dual demand/claim is reflected in experience in the way theoretical suppositions are used to relate sense-data and the dependence of the sort of sense-data that arise on the external event. The report is a response to the perceived gestalt that gives the external event. Hence the observational statement that is made in response to the event is both a demand and a claim about the representation of the event. I shall call such a demand/claim an expectation.

This dual aspect of observation reports as both demands and claims about representations is most clear when we consider how language is originally imparted. Take the situation of a child being taught language. The mother says, pointing to an external event, 'The rose is red'. In one respect the mother is making a claim about how something in the world can be represented - namely that the object indicated can be represented by the sign 'rose' and that its color can be represented by the sign 'red'. In another respect the mother is transcending the informative context. She is making a demand on the child - namely that

that object concerned should be represented by the sign 'rose' and that its color should be represented by the sign 'red'. More simply the mother is making the claim that the event referred to can be represented by the expression 'The rose is red', and the demand that it should be represented by that same expression. She is imparting a demand/claim or an expectation to the child.

This expectational view of observation statements has to be contrasted with the traditional descriptive view. In the traditional view the process of establishing the linguistic conventions involved in the use of language was sharply distinguished from the process involving the use of this language to represent events. Language is first established conventionally and then it is used to report on events in the world. Thus what occurs simultaneously in the process of acquiring language - i.e. obtaining information about the world and learning how to code this information - was separated into two distinct temporal phases. It is assumed that one first learns how to code information about the world by learning the conventions of language; one then uses these conventions to express information about the world. More simply put, this traditional view holds that one has to first learn the meaning of words before one can use these words to impart information. This is such a deep-seated assumption that the expression 'Define your terms before you begin your discussion' appears to be the most innocuously acceptable requirement in any debate.

The expectational view of statement collapses this dichotomy - learning how to code information about the world simultaneously involves learning about the world. The information coded and the code are given together. The process of institutionalizing the network of demands on

how the world should be represented is itself the process of imparting information about how the world can be represented. With the exception of the terms of formal systems that have nothing to say about the world all procedures that give meaning to terms also make empirical claims; and all empirical claims also define the meanings of the terms in which they are made.

Let us examine what is implied in the view that all observation sentences are expectations. On the one hand, it means that the sentence can be treated as imparting information about the world. This it does by showing that the event in question can be represented in a particular way. On the other hand, the sentence can also be treated as instituting a demand on how the event should be represented. This involves a demand on how the terms of the sentence should be used to represent the event, i.e. it involves an implicit convention about how the terms are to be employed. These dual aspects of a sentence cannot be separated - they are complementary aspects of one expectation.

This has far reaching implications for any theory of meaning. For the introduction of any observation report into a language from which it cannot be deduced - and this would be so if the report is about an event that cannot be predicted or deduced from the system - injects a subtle shift in the meanings of nearly all the terms of the language. The observation report itself changes somewhat the meanings of the terms in which it is formulated because it implicitly defines the terms contained in it. Since these terms are also linked to other terms of the language the changed meaning is propagated throughout the system as a whole. This surprising view that every sentence of a language affects the meanings of all terms in the language will become more evident after we have

developed an adequate theory of meaning.

If the empirical claims embodied in observation reports also confer meaning on the terms used to formulate them, we should allow that linguistic conventions also embody empirical claims. The two are indistinguishable - or rather they are double aspects of one expectation. Many examples from the history of science illustrate this. Temperature was defined in terms of the expansion of mercury at one time, and later it was claimed that it has been found that mercury does not expand uniformly with temperature. The planets were defined in Ptolemaic astronomy as a class of heavenly bodies - the sun, the moon, mercury, venus, mars, jupiter and saturn. After the Copernican revolution it was claimed that the sun and moon had been discovered not to be planets.¹⁶ The elements were defined as earth, air, fire and water in Aristotelian chemistry and subsequently it was claimed that they had been discovered not to be elements. Mammals were defined as warm-blooded land animals and later biological research showed the whale to be a mammal.

There appears to be a paradox here. If to establish a convention is merely to indicate how certain terms are to be used, how can such conventions be considered to be empirically disconfirmed? However, if all that was involved in these changes was a change of convention can it be said to be a significant discovery that mercury did not expand uniformly with temperature; that the sun and moon were not planets; that water was not an element; that the whale was a mammal? For surely, unless we accept the expectational view, a change in our language cannot be said to constitute a discovery about the world.

Take the case of the planets. Before the Copernican revolution a statement like 'The sun is a planet' would have appeared trivially true because the sun was by definition a planet. Yet Copernicus' great discovery was that the sun was not a planet, and that the earth was really a planet. Thus the statement 'The sun is a planet' was considered disconfirmed by Copernicus. How could this be the case if all that Copernicus achieved was to change a linguistic convention? It seems to me that the problem arises from the attempt to separate linguistic conventions and empirical claims. I want to assert, instead, that linguistic conventions also embody empirical claims, and that we cannot distinguish between the two.

Thus one may define a planet in Ptolemaic astronomy as "A body that wanders against the background of fixed stars". Then it would become empirically observed that the sun, moon, Mercury, Venus, Mars, Jupiter and Saturn were planets. On the other hand, one can first define these bodies as planets. Then it would become empirically observed that they wandered against the background of stars. Thus using one expression as a definition we obtain another as empirical. However, this is to distinguish artificially between two similar sorts of statements. Why not consider both of them to be simultaneously defining conventions and empirical claims - i.e. 'Planets are bodies that wander against the background of stars' and 'The sun, moon, Mercury, Venus, Mars, Jupiter and Saturn are planets' are expectations?

If linguistic conventions are also empirical claims it becomes obvious why they can be said to be empirically disconfirmed. Even if a convention appears to be purely a demand on how the world should be represented, it is also a claim that the world can be represented in the

way it prescribes. This is clearly seen if we recognize that to impose a system of linguistic conventions on the world - even so elementary a system as classifying the events in the world - is to represent our world in a specific way. The world may turn out to be such that we find that we cannot apply our conventions consistently. We may have a network of theories that suggests that the whale is a mammal and a system of classification that says that all mammals are land creatures. To achieve consistency we have to reject this conventional classification and introduce a new one. What is this but to treat the convention as being empirically revocable? Surely, this is to deal with the convention as an empirical statement.

The view that we should distinguish between defining conventions and empirical claims is a highly artificial one. It belongs to a particular philosophy and does not reside in the nature of statements themselves, or in the way that scientists treat statements. It has no divine right to immunity from criticism or rejection. Actually, if all that has been said above is accepted the distinction as one between different types of statements is untenable. All linguistic conventions exhibit a double aspect - they are also empirical claims.

Consider the notion of a 'straight line'. We can make the following statements regarding it:

1. A light ray path is in a straight line
2. A stretched cord makes a straight line
3. A body far from other bodies travels in a straight line

If we adopt the sentence (1) as a convention defining a straight line then the statements (2) and (3) can be viewed as empirical observa-

tion reports. However, we are equally free to select (2) or (3) as defining the convention for a straight line. Then the other two statements become empirical assertions. The fact is that there is no intrinsic difference in the nature of the statements (1), (2) or (3) that allows us to select one as conventional and the others as empirical observation reports. The distinction we make is arbitrary. Actually the statements of themselves are neither only conventional nor only empirical. They are such that they can be treated either as conventions - as demands on what the term straight line should be used to represent - or as claims - what can be represented by the term straight line. In this sense the statements are expectations.

Not only are linguistic conventions empirical claims but, if the expectational view is correct, we should find that any observation report could also be dealt with as a linguistic convention. The history of science also offers cases where this has occurred. For example, pendulums were originally found to exhibit a periodic regularity as measured by water clocks; later they were used to define periodic time intervals. This meant that the empirical discovery of Galileo was treated as a convention for measuring time. Similarly Einstein used the empirical law of classical physics that light travelled at uniform velocity as a convention to measure temporal intervals. Scientists measure time and length in many different ways. Why should we select one of these ways as a convention defining these terms, and treat all the others as empirical claims? Such a distinction is totally arbitrary. Why not simply assume that all of them are open to being treated either as conventions or as empirical claims, but that none of them is merely a convention or an empirical claim? I.e. why do we not merely view all

of these different procedures as involving expectations?

Logical empiricist philosophy makes a distinction between co-ordinative definitions and empirical claims. Thus it assumes that in geometry the process of giving, say, uninterpreted Euclidean geometry a physical interpretation requires one to adopt the convention that the path of a light ray defines a straight line. A physical point is defined as the intersection of such light rays. Physical triangles, polygons and circles can be defined in terms of straight lines and points. By means of such co-ordinative conventions it is held that the theorems of geometry can be made to represent the properties of physical space.

Let us examine more closely the co-ordinative definition that is established by defining a straight line as a path of a light ray. Is this definition merely a linguistic convention? There are some philosophers, like Poincare and Reichenbach,¹⁷ who have considered that this is so. However, it can be argued that they are mistaken. For the term 'straight line' even when it is an uninterpreted primitive is already involved in a network of relationships with other primitives. I.e. the terms 'straight line' and 'point' were defined to be used in a particular way with respect to one another in the uninterpreted system, and the new co-ordinating definition of the straight line in terms of the path of a light ray is an extension of the meaning of the term 'straight line' which was already defined relative to the other primitives.

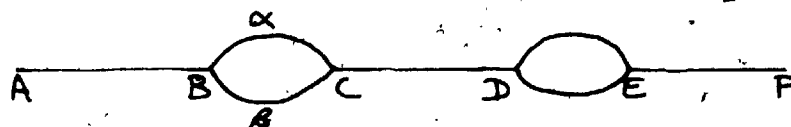
For this reason the new co-ordinative definition is more than a linguistic convention. One has only to recognize that, as a matter of empirical fact, the new definition may turn out to be incompatible with the way 'straight line' had been defined in the uninterpreted system. It may be found empirically that if we interpret the straight line as

the path of a light ray, then the sum of the angles of the triangle is not 180° . But there is a theorem in the uninterpreted Euclidean system that says this sum is 180° . Hence there may be grounds for rejecting the co-ordinative definition. Thus the statement 'A straight line is the path of a light ray' cannot be taken as a mere linguistic convention.

Nevertheless, even if it is the case that the co-ordinative definition is affected by empirical states-of-affairs, it is not the case that the statement "A straight line is the path of a light ray" is purely an empirical one. This is because it is only after the co-ordination of the primitive terms to the world that the geometry can make empirical assertions.

Hence, in co-ordinating 'straight line' to 'light rays' we are neither making only an empirical assertion, nor adopting a purely linguistic convention. The co-ordination is in one sense the demand that the term 'straight line' be considered to be the path of a light ray, and in another sense it is the claim that the path of a light ray can be represented by a straight line. It is a demand/claim or expectation that the path of a light ray is in a straight line. We cannot ignore either the prescriptive or the descriptive aspect of the definition. The descriptive aspect is indicated because empirical observation can make the definition untenable. The prescriptive aspect is indicated because the statement is the first co-ordination of the primitives of the uninterpreted system to the world.

To illustrate my point, imagine that the path of a light ray is as follows:



The path along AB gives a unique ray which then divides into two parts which come together at C and so on. If we define the straight line of Euclidean geometry by co-ordinating it to such a light ray we are going to find that it will not work for we would have to suppose that a straight line has segments that divide and come together to enclose a space. Furthermore, even if we consider α and β to be separate light rays (so that actually the segment AB could be interpreted as two overlapping light rays) we still need to suppose that two different light rays (straight lines) could contain two common points. This is a claim that may be true for the straight lines of Riemannian geometry but cannot be true for Euclidean geometry. Hence, co-ordinating the term 'straight line' of the uninterpreted Euclidean geometry to the physical path of light rays already involves presupposing that light rays conform to certain forms of behaviour.

It is misleading to view the co-ordination of the primitives to reality as merely conventional. Calling it a convention suggests that it is arbitrary - the connection of a mere sign to an external referent - but the sign itself is already constrained somewhat by the meaning it has been given in the uninterpreted geometrical system. Thus even with the first co-ordinative link we establish between the primitives of the axiomatic system and experience, we are already involved in empirical expectations about reality. These co-ordinative definitions are as much expectations about reality as any other theoretical statement. This is obvious if we consider the fact that there is already a preestablished network of structural relationships between the primitives of the uninterpreted system that must be preserved by the interpreted system if it is to be a viable representation of reality. However, whether it

will be preserved by choosing a particular co-ordinative definition is an empirical matter.

The point made is a general one that applies to all co-ordinative definitions. Such definitions are both linguistic conventions as well as empirical claims. They are expectations that reflect the double aspect of being simultaneously descriptive and prescriptive. Consider the Bohr model of the hydrogen atom. Nagel¹⁸ argues that to connect the model's prediction to empirical reality it was necessary to link the predictions of the theory by means of co-ordinative definitions to the results of observation. To do this the co-ordinative definition employed was the following: the theoretical expression, 'X is the wavelength of the radiation emitted when an electron jumps from the next-to-the-smallest to the smallest permissible orbit of the hydrogen atom', was linked to an experimental expression, 'Y is the line occurring at a certain position in the spectrum of hydrogen'.

Is this really a definition? Does it not presuppose the theoretical claim that the wavelength of light at the line Y would be X? Clearly it would not be a difficult task to measure this wavelength - it could be easily done by using a spectrometer. Furthermore, it was the knowledge that the line Y had the wavelength X that lead to co-ordinating it to a particular electron transition. Should it turn out that the wavelength of the light at Y were not X, the co-ordinative definition would be eliminated. Thus the co-ordinative definition really involved a theoretical claim - namely that the radiation emitted by the transition of the electron from the next-to-the-smallest to the smallest orbit was the radiation observed at Y. This theoretical claim can be disconfirmed should it turn out that there was an experimental error that lead

to a false determination of the wavelength of the radiation at γ .¹⁹

The view that co-ordinative definitions are merely conventions appears to ignore the problem raised by possibility of the multiplicity of ways in which a theoretical expression can be linked to an experimental situation. As Nagel himself points out the theoretical concept of 'electron transition' in Bohr's theory could also be connected via Plank's radiation law (which can be deduced from Bohr's theory) to the changes in the temperature of black-body radiation. Thus the concept can be defined in two different ways. What grounds are there for supposing that these two definitions are compatible? Or, if they are, how do we explain the fact that they are? But if we choose only one of them as the co-ordinative definition on what grounds do we explain the rejection of the other? Considering co-ordinative definitions as expectations does not raise such problems. Being expectations they are also empirical claims, and the fact that different co-ordinative definitions agree is an empirical discovery.

The problem of the overdetermination of the so-called theoretical terms because they can be linked to observation in a number of different ways has always been a difficulty for logical empiricists.²⁰ It was difficult to explain the possibility to adopting any of a number of different co-ordinative definitions all equally acceptable. It is also a problem for those who wish to adopt an instrumentalist view of such terms. How do we account for the remarkably correlated results obtained by adopting any one of a number of co-ordinative definitions? Many of the notions of science like temperatures, length, time, wavelength or chemical element can be linked to experience in a large number of ways. Each of these co-ordinations cannot be supposed to be merely definitions.

- they are as much empirical claims as they are definitions. It is true that without such co-ordinations we could not connect our theories with empirical reality, but this does not mean that they are purely linguistic conventions. They are expectations. Being expectations they are empirically corrigible.

The theory-ladenness of even what may appear to be merely linguistic conventions is most clearly revealed when we consider those situations where someone is taught to speak a theoretical language purely ostensively. Take the case of someone who is taught to read bubble chamber photographs purely ostensively. He could learn to identify the tracks of protons, mu-mesons, pions and many other elementary particles without any knowledge of theoretical physics at all. In fact this has been done in many centres of research involving high energy physics where laboratory technicians are taught to identify and measure the properties of particles from tracks observed on photographic plates. Such learning bears a close resemblance to the way we first acquire language. Confronting James' "booming buzzing confusion" in the photographs the technician is given a language to order this world, and this language is taught purely as a system of conventions. Nevertheless, it is clear that the 'conventions' he is being offered are themselves the result of a sophisticated network of theories about the world. The only reason they appear to be merely linguistic conventions is because, in this context of learning, they are treated as labels to attach to different sorts of events that are present. What is actually learnt is a way of classifying events that is highly theory-laden. Even though he is apparently learning how to use words he is also acquiring implicitly a theoretical structuring of reality. But, from the point of view of the

technician a report like 'A proton has crossed the bubble chamber' may appear as theory-free as 'The dog has crossed the room'.

Is this not how we learn language at the beginning of our lives? Are we not also taught how to attach symbols to events in the world? Do we not thereby also acquire an implicit theoretical structuring of the booming buzzing confusion that we first confront? It is language that gives form to the shifting sensations through which the world offers itself - perception organizes the sensory raw material into gestalts that make sense of the external world. Language in offering us what appears to be nothing more than the way to use words also gives us a theory about the world: a theory that tells us how to exploit the information in sensory indicators, and how to structure the chaos of sensations into a world of perceived gestalts. Even the learning of our first words in an object language like 'table', 'tree', and so on is as much the learning of perceptual gestalts constructed around sensations as the learning of 'proton', 'mu-meson' for the technician. The most basic conventions of our language are as much theoretical and empirical claims as any other we are likely to learn.²¹

This means that we cannot separate an observation language from a theoretical language. The view that there is a pure observation language is characteristic of atomic empiricists. At first they thought that such a language could be constructed solely on the basis of terms that referred to sense-data; difficulties in actually constructing such a language led them to abandon this program and appeal to a language that referred to physical objects that could be directly observed. However, neither approach can succeed for all linguistic conventions are also, as we have seen, theory-laden. The teaching of such conventions does not

involve appeal to sense-data but to perceptual gestalts offered in experience, and for which sense-data are only the occasion. The nature of such gestalts is intimately affected by the way we organize raw sensation into units of experience: a process that is affected by the theories we bring along to observe the world.

This clearly means that we have to give up the traditional distinction between theoretical and observational terms. All terms are theoretical because even terms that are given meaning by direct empirical associations appeal to perceptual gestalts constructed around sense-data. Being theory-laden the way such terms are used can be revised on the basis of empirical data. Even a predicate like 'red' may be retracted: one may recognize that a red star is not really bloody but only appears so because it is receding.²²

Thus, even the most elementary and entrenched conventions of a language are theoretical in nature and embody empirical content. Their stability is only due to the stability of the common sense theories of which they are a part: theories tested over millenia of human history and upon which subsequent theoretical knowledge is erected. The perceptual gestalts to which commonsense knowledge appeals to are no less immune to revision and are as much theoretical as those experienced by physicists confronting bubble chamber photographs; doctors examining X-ray evidence; geologists identifying rock formations; astronomers labelling stellar nebula and biologists classifying organisms.

The untenability of the distinction between theoretical and observational terms means that we cannot make a distinction between theoretical and observation statements. All observation statements are also theoretical. In the atomic empiricist view observation statements are

considered to be somehow given, whereas theoretical statements are deemed not to possess this foundational stature. Connected to this is also the assumption that observation statements are supported by single acts of observation, whereas theoretical statements appeal to a class of observations for support. Furthermore, observation statements are held to have an intimate connection with reality that theoretical statements lack. All these claims are unfounded. One cannot make any of these assertions the basis for a significant demarcation between observational and theoretical statements.²³

The thesis of the givenness of observation reports is untenable. The report of an event in reality is intimately affected by the observational theories used in formulating the report. It is in this sense that we have claimed that observations are theory-laden. Hence the argument based on the givenness of these reports cannot be used to make the distinction between theoretical and observational statements.

Neither can we assume that observation statements are given because they are not capable of being rejected on a later occasion. A change of theories may lead us to reformulate an observation in a manner that is logically incompatible with the original formulation. E.g. we may reject the report 'The sun rises in the sky' as an observation report when we adopt the Copernican theory in place of the Ptolemaic one. Thus, like theoretical statements, observational statements lack a givenness both in the way they arise and in being immutable to reformulation or rejection.

Nor is it true that observation statements are supported by a single act of observation, whereas theoretical statements are not. The theories used to constitute the report are supported by other classes of

observations. Therefore, the report is what it is because of these other classes of observations. Through the intermediate connection provided by the theories used in making the observation, the observation itself may be deemed to be made in the context of all these other observation reports. The report would not be what it is - i.e. it would not be formulated in the way it is - if these other classes of observations had been different. It is only if we assume that observations are made in a theory-free context that we can justify the claim that they are supported by a single act of observation.

Neither is there any credibility in the claim that observation provides direct support to observational statements whereas theoretical statements are only indirectly supported by observation. It is not clear what direct support can mean if we assume that observation reports are not uniquely given by the event. The event is merely the cause of a complex of sense-data which are sensed along with a whole complex of other sensations not causally linked to the event. To see the relevant sensations as indicators and to construct the appropriate gestalt for the event also involves us with the theories employed to observe. Thus the support for the report depends not only on the event, but also on the support these theories possess. In this case one cannot reasonably suppose that observational expectations are somehow more directly supported by experience than are theoretical statements.

Let us now examine the contention that observation statements are somehow more intimately connected to reality than are theoretical statements. What can this claim amount to? It cannot be based upon the fact that observation statements refer to events in reality for their support, since theoretical statements also do this. Neither can it be that

observation statements are indubitably given by events for we have seen that this is not the case.

Could it be based upon the fact that theoretical statements appeal directly to observation statements for their support whereas the converse situation does not obtain? This certainly is not the case. For observation statements do the converse when they appeal to theories in their formulation. Furthermore, even if theoretical statements are referred to observation statements for support, it is not always the case that theories are passively dependent on such reports. Often theories force us to reject such reports by revealing inadequacies in the way the report is formulated.

It is obvious that there is no fundamentally significant distinction between observational and theoretical statements. The demarcation between two such classes of statements cannot be made. All observation statements are also theoretical. This is only to be expected if all experience is theory-laden. The gestalt view of perception requires us to suppose this. If experience offers us perceptual gestalts organized around sense-data, and observation reports appeal to such gestalts, then all observation reports have to be theoretical.

Nevertheless, there is some substance to the motivation of traditional philosophers who attempted to make this distinction. The distinction, however, is between statements that are reports of perception and other statements that are not direct responses to experience. Observation statements are reports; these other statements are nonreportive. Nonreportive statements appeal to reportive statements for empirical support, but this does not require us to suppose that these reportive statements are not theoretical. All the statements of science are

theoretical - reportive as well as nonreportive. Since the theory-ladenness of reportive or observation statements requires as to view them as expectations, we should expect nonreportive theoretical statements to be expectations, too. This is what we actually find.

1.4 Nonreportive Theoretical Statements as Expectations

Nonreportive statements exhibit varying degrees of generality. At the lowest level are statements like 'Copper expands on heating'. Slightly more general are statements that express low level laws like Hooke's law which tells us that the extension in a spring is directly proportional to the tension placed on it; or propositions like 'All mammals suckle their young'. Of even greater scope are Kepler's laws of planetary motion, the laws of combining weights in chemistry or Weber's law in psychology. Finally there are those statements of wide applicability that express the law of conservation of energy; quantum electrodynamics; Darwin's theory of evolution by natural selection; or Newton's theory of gravitation.

There are a number of reasons for supposing that all such nonreportive statements are expectations. This is because though the statements are empirical any one of them can also be treated as prescriptive and immune to revision. In the first place there are the conventionalist arguments presented by the mathematician and philosopher Poincare at the beginning of this century which show that any theoretical statement can be protected from disconfirmation by treating it as a meaning postulate that implicitly defines the terms in which it is formulated. Secondly, there is the argument offered by Duhem and developed by Quine that any statement may be shielded from empirical refutation if we are prepared

to make a sufficiently drastic alteration in the rest of our conceptual system. The Duhem-Quine thesis rightfully points out that any prediction always involves more than one theoretical premiss. Thus a failure in prediction can always be resolved by modifying premisses other than the ones we wish to treat as prescriptive. Thirdly, it has been aptly suggested by Kuhn and Feyerabend that even if a theoretical statement appears to be in conflict with an observed result the blame could always be shifted to the observational theories used to get the result. The Kuhn-Feyerabend thesis allows one the option of rejecting an observation report that disconfirms a theoretical prediction.

Thus any theoretical statement may be treated as an empirical claim, or it may be defended against empirical disconfirmation by various strategies. By doing the latter systematically one would treat the theoretical statement as a prescriptive demand on how events in its domain should be represented. Alternatively, by treating it as a statement open to rejection it is viewed as a claim about how events in its domain can be represented. Being open to treatment either as prescriptive or descriptive the theoretical statement is an expectation.

The debate between the conventionalists and the empiricists in the early years of this century illustrates the dual aspect of theoretical statements required by the expectational view. The conventionalists wanted to interpret all (nonreportive) theoretical statements as 'disguised definitions'²⁴ of the terms involved in them or as principles that were essentially immune to experimental disconfirmation and used to organize observed data. Thus they wanted to treat them as prescriptions or demands on how the events in the world should be represented. The empiricists countered that these statements were empirical claims, and

therefore observationally testable. They were both right in what they affirmed and wrong in what they denied. For being an expectation-every theoretical statement is open to being treated either as an analytic definition or as an empirical claim.

Consider the conventionalist case for geometry presented by Poincare. He argued that every system of applied geometry was a set of 'concealed definitions' or 'conventions' for measuring spatial relations. Therefore geometry was not an empirical science. Poincare was profoundly affected by the discovery of the non-Euclidean geometries by Bolyai, Lobachevsky, Riemann and Gauss, as well as the pervasive neo-Kantian views of his time. He presents a number of arguments to substantiate his position. He says that geometry is not synthetic a priori in the Kantian sense or else Euclidian geometry would impose itself on us as the only possible one. We could not conceive of the existence of non-Euclidean geometries. This, however, does not mean that Euclidian geometry is an empirical science. Empirical sciences cannot be exact but Euclidian geometry is an exact science. According to him the geometrical axioms, being neither synthetic a priori nor empirical, have to be conventions. Euclidian geometry applies to the world as an exact science because we have chosen it freely as a convention in terms of which to interpret all observed results. As he says:-

"Our choice among all possible conventions is guided by experimental facts; but it remains free ... In other words, the axioms of geometry ... are only definitions in disguise."²⁵

According to Poincare it is the amorphousness of space that allows us this freedom to choose geometrical conventions. Space allows us to draw triangles the sum of whose angles is two right angles; but it also allows us to construct triangles whose angles sum to less than two right

angles. The sides of either sort of triangle could be defined as straight but nothing in the nature of space dictates this choice. It is our free selection from among the possible alternatives that makes space conform to Euclidean or one of the variants of non-Euclidean geometry. By the conventional choice of geometry we also determine conventionally what we wish to consider to be a straight line in space. For him this choice is equivalent to the choice of determining how we intend to measure lengths.

"Are there lengths expressible in metres and centimetres, but which cannot be measured in fathoms, feet and inches, so that experience in ascertaining the existence of these lengths would directly contradict the hypothesis that there are fathoms divided into six feet."²⁶

To further support his conventionalist assertions Poincare uses an alternative approach that is an interesting illustration of Duhem's critique of the possibility of crucial experiments in science. He points to the impossibility of refuting a geometry by appealing to empirical evidence. Suppose one designed an experiment to decide between Euclidean and Riemannian geometry by measuring stellar parallax. Riemannian geometry predicts that some stars would exhibit negative parallax, but Euclidean geometry leads us to anticipate no parallax. If we should observe negative parallax we cannot conclude that there are grounds for rejecting Euclidean geometry. For the experiment involved a test not merely of geometry alone, but of a geometry in conjunction with a theory of optics. This means that we are free to explain the negative parallax either by renouncing Euclidean geometry or modifying the laws of optics so that light would no longer be considered to travel in a straight line.

Nevertheless, Poincare believed that even if empirical science did not justify us preferring one of the possible geometries to the others, there were good grounds for adopting Euclidean geometry. In the first place Euclidean space conforms to our mental habits and we have direct intuition of it. Secondly, it is the simplest of all the geometries and is the most convenient to employ. This led him to hold that it was unlikely that Euclid's geometry would ever be replaced by its more recent competitors.

Poincare's conventionalist thesis has been vigorously attacked by the logical empiricists who failed to come to grips with his crucial insight. One of the earliest major critics was Reichenbach²⁷ who allowed that negative parallax could be explained without discarding Euclidean geometry if light did not travel in straight lines. This deviation would then be explained by the existence of special forces that acted selectively on light rays so as to deform their paths. If evidence for such differentiating forces were shown to exist then Euclidean geometry could be saved. However, it may turn out that measurement reveals no evidence for such force-fields. The only way that Euclidean geometry can be preserved now is to suppose the existence of universal forces that not only distorted the path of light rays, but deformed every material body whatsoever. This would explain why the forces were unobservable: like forces that cause all bodies in the universe to shrink to half their original size, these universal forces would leave observable states-of-affairs unchanged.

Reichenbach views the postulation of such universal forces as ad hoc because the only grounds for asserting their existence is to defend Euclidean geometry against experimental disconfirmation. He wants to

exclude all universal forces on methodological grounds as essentially adhoc. Also, even if Poincare was correct in supposing that Euclidean geometry was simpler than the non-Euclidean geometries it may turn out that the total system of physical theories with Euclidean geometry would not be simpler than one that employed a non-Euclidean geometry. An example is Einstein's theory of gravitation which can be expressed more simply if we allow a non-Euclidean geometry for space, but can only be formulated using Euclidean space at the price of invoking universal forces and increased mathematical complexity as a whole. The geometry of the system may be simpler than that in Einstein's theory, but the system as a whole would not be.

Reichenbach's grounds for objecting to Poincare's conventionalism are mistaken. His methodological dictum against universal forces would exclude gravitation from Newton's theory. The concept of a universal force cannot be meaningless because gravitation is precisely such a force: it acts alike on all bodies and it cannot be screened. If adhocness were involved in postulating such forces then Newton's theory of gravitation would have to be excluded on the sort of methodological grounds Reichenbach invokes.²⁸

Reichenbach does not address the issue of simplicity raised by Poincare either. The fact that a total system of theories may be simpler if one adopted a non-Euclidean geometry does not invalidate Poincare's claim that Euclidean geometry is the simplest, but only attacks his belief that it would always be the most convenient geometry in which to formulate physical theories. Einstein's discovery showed that Poincare was mistaken in assuming that Euclid's geometry would always be the most convenient but that does not mean he was mistaken in claiming that

Euclid's geometry cannot be falsified. These are two distinct claims and it is only the latter that plays a crucial role in Poincaré's case for conventionalism. This means that the postulates of geometry can be treated as analytic definitions of the term 'straight line'.

However, the fact that the postulates of geometry can be treated as analytical definitions does not require us to say, as Poincaré assumes, that they do not have empirical content. Applied geometry refers to the world, that is it offers us a representation of the events in the world. Though it can be treated as giving a network of conventions, it can also be rejected if this network is not the ideal one in which to formulate physical theories. Poincaré assumes that Euclid's geometry would always offer not only the simplest geometry but also be the one that would allow the simplest formulation of other physical theories. This assumption presumes something about the world - the world may turn out to be the kind of place where this assumption is untenable. Einstein showed that this was indeed the case.

Actually we cannot treat the postulates of applied geometry either as purely linguistic conventions or as empirical claims. They are both; they are expectations. This view of applied geometry is very different from the traditional interpretations that are offered. It rejects the Kantian view of geometry as expressing synthetic a priori truths because his notion of a priori (as distinct from his notion of analytic) requires us to assume that they are given and immutable. The expectational view also opposes the view that they are only conventions; or views like those of Mill²⁹ which requires us to view geometrical postulates as purely empirical generalizations induced from experience.

Not does it accept the traditional, and widely prevalent, atomic empiricist account of geometry.³⁰ This account sharply distinguishes pure and applied geometry. The former is concerned with analytic truths where the primitives in the axioms - point and line - are implicitly defined by the axioms. Applied geometry, on the other hand, is empirical and is obtained by giving the primitives of the uninterpreted system physical meaning. This could be done, say, by co-ordinating the notion 'straight line' to a light ray path and 'point' to the intersection of such paths. As we saw earlier such co-ordinative definitions are also empirical claims. Furthermore, if the uninterpreted system was a network of linguistic conventions why should we suppose this conventional aspect to vanish the moment the system is co-ordinated to the world? The theorems of an applied geometry are not synthetic a priori truths; neither are they only linguistic conventions or only empirical claims. These theorems exhibit the double aspect characteristic of expectations - they are simultaneously prescriptive and descriptive; linguistic conventions and empirical claims.

Being expectations we cannot separate a conventional component of these statements from an empirical one. The conventionalists can present their case successfully for any individual statement, but have to end up by supposing that such conventions can only be tested empirically by appealing to the system as a whole. The empiricists can defend their position, but only by saying that even if conventionalism works for a particular class of statements - e.g. when we consider geometry in isolation from other physical theories - geometry is empirically testable when we refer to the total system of our physical theories. Thus, by appealing to the total system the conventionalist is able to preserve

empirical significance. By appealing to the total system the empiricist is able to point out that the so-called conventions are empirically testable. This appeal to the total system, however, serves to mask the fact that the issue between conventionalism and empiricism is one which is impossible to resolve because the theoretical statements they consider are actually both linguistic conventions and empirical claims at the same time. The point is not to resolve the issue but to dissolve it by refusing to consider theoretical statements as either only linguistic convention or only empirical claims. They are only neither; they are expectations.

This comes out most clearly when we consider the way in which Poincare considers these conventions to arise. Such conventions are empirical laws that are subsequently generalized and given a conventional, and hence, irrefutable, status.

"Principles are conventions and definitions in disguise. They are, however, deduced from experimental laws, and the laws have, so to speak, been erected into principles to which our mind attributes an absolute value."³¹

He illustrates this by reference to Newtonian mechanics. Though Newton's three laws of motion may have begun as experimental laws they had subsequently been given the status of conventions. Thus Newton's first law of inertia does not tell us the behaviour of a body with no forces acting upon it: the law is actually a definition of such a body. Similarly the second law of motion defines force as a product of mass and acceleration; and the third law allows us to compare masses by specifying the ratio of two colliding masses. This process of implicit definition, according to Poincare, explains why the principles can never be refuted by experiment even though they are of empirical origin.

Nevertheless, the tension between empiricism and radical conventionalism is evident in Poincare. He answers the problem of how to make certain that the method is not misused if principles can be rendered unfalsifiable by convention on pragmatic grounds.

"Simply when it ceases to be useful to us - i.e. when we can no longer use it to predict correctly new phenomena. We shall be certain in such a case that the relation affirmed is no longer real, for otherwise it would be fruitful; experiment without directly contradicting a new extension of the principle will nevertheless have condemned it."³²

Thus Poincare does not consider his conventions to be immune to empirical pressure. In fact, even though they are implicit definitions of the terms involved they should also be capable of representing reality. They prove this by their fruitfulness - their ability to predict new phenomena. Even if their conventional status prevents them from being directly contradicted by experiment, they nevertheless remain open to empirical condemnation if they cease to be fruitful.

Clearly there is a great deal in common between the expectational view of theoretical statements and Poincare's view of principles. His principles are both demands on how events should be represented (linguistic conventions) and claims on how events can be represented (their fruitfulness makes them empirically testable). It is true that Poincare viewed them as conventions but his use of the term did not preclude them from being empirically corrigible. Thus even though they functioned as meaning postulates they also conveyed empirical content. But there is one basic difference that precludes us from identifying his conventions with expectations. For Poincare conventions begin as empirical statements but they cease to be such when they are accorded the status of principles. Though the same statement may be deemed conventional or

empirical he does not allow it to be both at the same time. The expectational view rejects this position - the reason why empirical laws can be treated as conventions is because all statements are simultaneously linguistic conventions and empirical claims.

The view that theoretical statements are expectations resolves the difficulties found in treating them as purely conventions or as expressing only empirical content. Take the case of the law of conservation of energy. This law states that 'In all the processes of nature energy is conserved'. It was first formulated for mechanical systems in which only two kinds of energies were involved - kinetic and potential. It bore a resemblance to another conservation law - the law of conservation of mass - that played such an important role in chemistry, and later in physics. When the law was first formulated it was clearly not regarded as a linguistic convention. Later it was discovered that the law was not always applicable, particularly in those processes in which frictional forces were involved. To preserve the law Joule proposed that heat was a form of energy, and mechanical energy was changed into heat energy where friction was involved. Assuming that the law held Joule was able to experimentally arrive at a measure of the mechanical energy that was equivalent to a given unit of heat energy - the mechanical equivalent of heat. Clearly, in this approach, the law was used as a means of defining the concept of heat energy. I.e. the law was treated as a convention regarding energy, and the measure for heat energy was devised to preserve this convention.

There could have been two alternative approaches to dealing with the problem generated by frictional processes. One would have been to treat the law as an empirical claim that was refuted by observation

involving frictional forces - or that was only valid for processes that did not involve friction. The other was to keep the law, but use it as a convention to define heat energy. In the latter instance the law becomes a meaning postulate for extending the meaning of 'energy' to include heat.

Thus the law of conservation of energy was open to being treated either as expressing matters of fact or as a meaning postulate for the term 'energy'. In fact it was open to both sorts of interpretation. If we treat it as an empirical law it becomes a claim about the way reality is to be represented. If we treat it as a convention it becomes a demand about the way reality is to be represented. Thus, like observation reports, it is a demand/claim about the way to represent reality. In short, it is an expectation about reality.

It was by such a process as the above - where the law of conservation of energy was employed both as a defining convention and an empirical claim - that it was extended to all sorts of physical processes. It came to include not only mechanical and thermal events, but also electrical, magnetic and chemical phenomena. Einstein extended the concept to include even mass energy by treating the law as a defining convention. For he had to suppose that the law of conservation of energy held in order to derive his famous expression for the energy contained in material mass. Nevertheless, even if the law were treated as a convention for defining new forms of energy this does not mean that it is only a convention. Whether the convention can be successfully extended to define new forms of energy also depends on the nature of reality. Einstein could use the law to define mass energy but only because the nature of the world he confronted allowed this. Such a definition would

not have worked in a Newtonian world because there mass could not change to energy. It would have involved a violation of the law of conservation of mass.

The expectational view does not apply only to observation reports and high level generalizations like the axioms of geometry, the principles of mechanics, or the law of conservation of energy. Even low level nonreportive theoretical statements like 'Copper expands on heating' and 'Whales suckle their young' are expectations. Should we confront a situation where it is observed that these direct generalizations from experience are disconfirmed we could adopt the conventionalist strategy of denying the status of being copper or being whales to the objects under study, and modify the rest of our conceptual framework to accommodate these assumptions. Of course we may not want to do this because of other reasons, but without these external considerations there is nothing in the nature of the statements themselves or the way they arose that prevents us from treating them as conventions. We have seen examples where this actually occurred - e.g. Galileo's law of periodic motion for pendulums and Einstein's principle of invariance of light velocity were both empirical discoveries that were subsequently employed as conventions for measuring time. Similarly the empirical observation that 'Mammals suckle their young' is now employed to define the class of mammals. The fact that not every nonreportive statement is treated as conventional does not mean that every such statement is not open to such treatment. The reason that this is possible is because all nonreportive statements possess the double aspect of being conventional and empirical at the same time - i.e. of being expectations.

1.5 Conclusion

The expectational view of all theoretical statements - both reportive and nonreportive - differs radically from the traditional atomic and holistic empiricist interpretations. The atomic empiricists divide nonreportive statements into two exclusive classes - conventional and empirical. This was the view of Poincare who, whilst recognizing that conventions were originally empirical laws that had been erected into principles, did not allow the same statement to be both conventional and empirical. Statements that were given conventional status ceased to be empirical. A similar position is held by Nagel. The latter holds that there could be different formulations of a theory all of which are logically equivalent, and that each formulation involves treating some statements as conventional. Furthermore, the statements chosen as conventional in one formulation can be treated as empirical in another. This is possible only because some statements treated as empirical in the first formulation are treated as conventions in the second. Thus for Nagel each formulation may divide statements into conventional and empirical in different ways though no formulation can treat all statements as only conventional or only empirical.³³

Nagel's account openly confesses that there are statements of a theory which can either be treated as linguistic conventions or as empirical laws depending on the context. However, Nagel is intent on separating these two classes of statements in any particular context. This is unnecessary. It is only a particular philosophy that requires us to do this. Physicists, for example, are not concerned with demarcating the meaning postulates of their theories from the statements expressing empirical content. Nagel supposes that a statement now

treated as a law may be treated as a convention provided other statements that are now treated as conventions are treated as expressing empirical content. This means that the distinction between conventional and empirical components of a theory becomes arbitrary. In fact, all statements of a theory are both linguistic conventions and embody empirical content and there is no utility in taking this dual aspect of every statement and projecting it onto two separate classes of statements. They are all expectations.

The holistic empiricists like Quine and Feyerabend are aware that statements cannot be divided into dual classes on the basis of the analytic-synthetic (or conventional-empirical) distinction. This collapsing of the distinction does not lead them towards the expectational view. Instead Quine adopts the position that all statements are empirical; and Feyerabend, on occasions, seems to take all nonreportive statements to be analytic. It is obvious that once the distinction is abolished there are two alternative approaches that are possible. However, Feyerabend's radical position is not compatible with his scientific realism or empiricism. Where he actually attempts to clarify his position it is evident that for him it is only the deepest principles of a theory that can be deemed to be analytic.³⁴ Nevertheless both Quine's radical empiricism and Feyerabend's conventionalism are equally untenable. They are two faces of the same coin. They are modern variants of the classical positions adopted by empiricists and conventionalists at the end of the nineteenth century but occurring after the untenability of the analytic-synthetic distinction between statements has been recognized. But neither recognizes that the reason that such a distinction between statements cannot be made is not because the distinction itself is untenable,

but because it is not a distinction between statements. It expresses the double aspect of all statements.

In his classic paper 'Two Dogmas of Empiricism' Quine appropriately criticizes the tenability of the analytic-synthetic distinction between statements. However, this leads him to argue that it is impossible to distinguish a linguistic and a factual component in individual statements and that this duality cannot be traced into individual statements.

"... it is nonsense and the root of much nonsense, to speak of a linguistic and a factual component in the truth of any individual statement. Taken collectively science has its double dependence upon language and experience; but this duality is not significantly traceable into the statements of science taken one by one."³⁵

We have seen that the expectational view requires us to do precisely what Quine rejects can be done - individual statements are simultaneously linguistic conventions and empirical claims. Both Quine and Feyerabend are correct - but only partly so - because each emphasizes one dimension of a double aspect that belongs to all statements.

In the case of observation statements there appears to be universal consensus among atomic and holistic empiricists that they do not have any conventionalist status. They only differ in respect of their corrigibility - for Feyerabend all observation reports are corrigible; for Poincare and Nagel they are not. Quine appears to equivocate on this issue and this ambiguity on his part will become explicable when we compare his theory of meaning to the one we shall develop subsequently.³⁶ In this respect the expectational view is absolutely at variance with the traditional standpoints - observation reports are like all other theoretical claims in being both linguistic conventions and empirical claims.

In this chapter I have argued that sense-data are indicators given to experience of events in the external world. Our experience of these events is of gestalt units constructed in the context of theories around sense-data. Thus all experience is theory-laden. The theory-impregnation of experience requires us to relinquish the distinction between observation and theoretical statements. Observation statements are also theoretical; they are reportive theoretical statements. Furthermore, all theoretical statements - both reportive and nonreportive - are expectations. They are both linguistic conventions that prescribe how events in the world should be represented and empirical claims that describe how events in the world can be represented.

It will become subsequently evident that the expectational view requires fundamental transformations of the very categories in which epistemological issues have been raised. It leads to a collapse of a large number of dualisms that have plagued philosophy since the debates between the rationalists and empiricists in the seventeenth century. It has implications as significant for epistemology as the wave-particle duality has for physics. For just as physics divided matter into two classes that exhibited wave-like and particle-like properties in the nineteenth century, so in philosophy we divide all statements into separate classes on the basis of properties that are shared by all statements. Recognizing the prescriptive/descriptive dual aspect of statements forces us to recognize a dual aspect inherent in many other epistemological categories. The duality ultimately traceable to experience - the dependence of experience on theory and the world - means that all these philosophical categories also have a double dependence on language and the world.

CHAPTER TWO

THE NORMS OF SCIENCE

2.1 Introduction

Classical empiricist and positivist philosophies of science considered science to be a value-neutral enterprise. Of course there was some concession to the realm of values. Norms like simplicity, explanatory power, predictive power, coherence and generality were considered to be part of the system of science. But coherence was a logical value - a value of reason. Explanatory and predictive power as well as generality were subsumed under the notion of empirical adequacy. Simplicity was the one norm that appeared to resist a total characterization under this notion. Hence, a great deal of effort was spent in characterizing simplicity in some way that could lead to an interpretation of it in purely empiricist terms. Apart from this, it was felt that science was totally value-neutral.¹

One of the most characteristic aspects of the critique of logical empiricism in the last twenty years has been the attempt to show that science is not value-neutral.² Partly the stimulus was derived from the work of anthropologists and social scientists of the late 19th and early 20th century who - adopting a 'value-neutral' position in the examination of cross-cultural phenomena - were amazed at how radically different were the norm systems employed by different societies in organizing their experience. Another part of the stimulus was derived precisely by criticizing social scientists and anthropologists who examined cultures outside their own within a framework of norms provided by their own culture and, hence, distorted considerably the reality they

confronted. Also political confrontations between the left and the right and the conflicting 'scientific' theories that were engineered by social and political scientists in the process; the acceptance and rejection of theories by fiat by ideological movements and totalitarian dictatorships, all contributed to the increasing disillusionment with science and a greater awareness of the role that values played in science.

This does not mean that the role of values was recognized solely as a result of such external factors. Darwin's theory of evolution had a great impact, not only in making credible the view that scientific theories were mutable like everything else in nature, but also the view that man's cognitive functions were instrumental to his survival, and not merely organs that assimilated truth.³ Freud, with his theory of the unconscious and his demonstration of the way hidden motives are rationalized and mystified, demonstrated that men could act out of motives that lie outside their consciousness. Even scientists at their most objective are creatures of the irrational and one cannot suppose that it is only pure objectivity that operates when they generate theories.⁴ Furthermore, the Marxist position that the arts, sciences and philosophies of a period were nothing more than ideological superstructures erected upon economic interests, contributed not a little to the view that science was hardly a pure and disinterested pursuit of truth.⁵

Even within philosophy itself there were factors that contributed to the dissolution of the concept of a value-free science. Kant's 'Copernican Revolution' and the subsequent explosion of neo-Kantian movements emphasized the tremendous role that man's mind played in constituting the reality he confronted. This movement in philosophy was

reinforced by the neo-Romantic movements that emphasized man as the creator of his reality. This resulted in a general movement towards subjectivity that many felt could only be carried through by undermining the notion of an observer-independent value-free science.⁶

The most distinctive feature of the recent views that deny a value-free science is their attack on the value-neutrality of the physical sciences. It has always been taken as debatable whether the social sciences could be value-neutral, but until recently it was assumed that, apart from norms like simplicity, generality etc., the physical sciences were value-free. Even the classical sociologists of knowledge, like Merton, tended to argue that science was affected by sociocultural and religio-metaphysical values but only within the context of discovery. According to them the justification of scientific theories, however, was achieved independently of any values except those that determined empirical adequacy. (Of course it was conceded that individual scientists could be affected by idiosyncratic values, but these were seen to be self-cancelling when the debate was conducted in an open scientific community.)

However, holistic empiricists like Kuhn and Feyerabend have argued that there cannot be a distinction between a context of discovery and a context of justification. They hold that the values and standards adopted by a scientific community are changeable and mutable; and that there cannot be any superarching set of norms to arbitrate between scientific communities that have adopted radically different conceptual frameworks. Hence even the acceptance and rejection of theories in the physical sciences inextricable and intimately involves appeal to socio-cultural and psychological norms that are often incommensurate with one

another.

This has led to what may rightly be called a crisis situation in contemporary philosophy of science. If values are necessary to arbitrate between theories, and these values themselves are contingent and mutable, how is it possible to engage in scientific debate or strive for consensus? For the very notion of debate presupposes that there must be some shared set of explicitly or implicitly held norms by which theories may be compared. The apparent absence of transcendent standards has led Feyerabend to characterize science as just another myth and Kuhn describes scientific paradigms as different ways of approaching reality that cannot be comparatively evaluated (in the same way that schools of art cannot be objectively judged).⁷

It has led others to attempt to understand changes in scientific norms from a sociological perspective. Perhaps an understanding of the social context in which theories are created, accepted and rejected could account for the values that are used in evaluating theories.

Those who have taken this sociological turn emphasize the fact that the internal rationality of science must ultimately be unable to account for those episodes in science that involve a radical realignment, not only of our theories about the world, but also of the norms that are used in comparing theories.

Others argue that these sociologists of science have embarked on a project riddled with internal contradictions. For if they presuppose that the values of a community are internalized into theories in the process of theory creation, they have to assume that the same thing could also occur to their theories. If they presume that such values are also involved in the acceptance and rejection of scientific

theories, this same presumption must also be true of the sociological theories that are put forward to account for scientific change. Hence they must assume that the acceptance or rejection of theory they put forward is also functionally dependent on the kind of values that are accepted by the community to which they belong. Even the theories that they create are partly constituted by the values of the community in which it is embedded. But sociology, even more than the natural sciences, involves many competing communities that maintain conflicting theories about social reality. How is the sociologist to arbitrate between the different sociological theories that may be put forward to account for a particular change of paradigm?

The sociologist may be prepared to accept this conclusion, but deny that it refutes his position. He could claim that it is precisely what one must be prepared to allow given his position. He could claim that he does not explain the paradigm change from any absolute point of view, but from the point of view of the community that shares his values. Hence his sociological explanation takes into account the fact that it is, itself, created and accepted in the context of a particular community of sociologists.⁸

This way out of the problem, however, is to forfeit altogether the quest for consensus through debate. One merely allows that an explanation is adequate so long as one can get some community to accept it. What does one do if a part of the community that one belongs to begins to reject one's theory? Does one redivide the community into a narrower group? How could one then say that there is a community at all? Also why should one not affirm a theory because it conforms to one's own norms even if one cannot defend it against anyone else's views because

one's own norms are highly individual and eccentric?

I hope that it is not imagined that I am parodying the situation. I am merely drawing out the consequences of such a position. If we were to adopt such a position as acceptable it would lead us to a continuous fragmentation of a scientific community until there are only individuals with their individual viewpoints - all equally legitimate. In fact, even those who have opted for a sociological explanation of value changes are aware of the above dangers. Sometimes they attempt to get around the problems engendered by claiming that the sociologist is not involved in constructing theories to explain these value changes in science, but in merely describing the process through which they occurred. No sociological theories are being compared - only sociological facts are being described.

Nevertheless, we have seen that in the natural sciences there can be no theory-free observation - all observations are theory-laden. How can the sociologist avoid theory in the more complex domain of the sociology of knowledge? It seems more difficult to concede that there are social facts waiting to be revealed than that there are pure facts for physicists to uncover. Hence one cannot claim that since a sociological explanation of value changes is merely descriptive that the sociologist does not presuppose theories in his enterprise. If he does presuppose theories then he has to show why his theories are to be preferred to those assumed by some other sociologist in dealing with the same phenomena.

Nevertheless, we cannot do without the sociological approach. There is now ample historical evidence that scientific theories internalize, to a great extent, the norms of the culture in which they are

developed. The kind of theories that are created are not only affected by the dominant cultural values, but also by the norms of the particular subgroup in that culture to which a scientist belongs; and even by the idiosyncratic norms that he may hold as a result of his religious upbringing or psychological history.⁹

Of course, it is possible to claim that this only happens in the context of discovery of theories. Perhaps in the context of justification there is no appeal to norms other than those of simplicity, predictive power or explanatory power or generality. This Reichenbachian distinction between the context of creation and the context of justification, however, does not solve the problem. As we shall see more clearly, in a subsequent chapter there is evidence that cultural factors are also involved in the context of justification of theories, and that these factors change with time, and are dependent upon the scientific communities involved.

At this point we appear to be caught between the horns of a dilemma - either we reject the view that cultural factors are involved in explaining the content of accepted scientific theories and thereby ignore a great deal of the historical evidence to the contrary; or we accept this view and deny that an internal rational evaluation of scientific theories is possible. However, to reject an internalist theory of rationality is to affirm that scientists are mistaken when they imagine that theories can be evaluated and compared without appealing to contingent sociocultural values which are not involved in the internal history of science.

Consider this dilemma more closely. In the first place scientists appear to require that theory comparison must be made in a value-free

atmosphere. In proposing scientific theories one is not allowed to explicitly appeal to sociocultural norms to support one's theory against a competitor that does not appeal to the same norms. If one were to make it obvious that one was appealing to religious, metaphysical or political norms one is considered biased or prejudiced. Hence, even if it is the case that science is not value-neutral, it is also the case that the scientific ethos prohibits the blatant appeal to idiosyncratic norms to support theories. Why is there this peculiar ethos? If science is not value-neutral why is it that scientists are not allowed to explicitly declare their values and refuse to consider theories that do not conform to them? Even those sociologists of science who reject a value-free science are often reluctant to acknowledge explicitly the sociocultural values they subscribe to. It would seem that a good reason to do this would be to allow other sociologists to know in advance what sort of sociological theories they are prepared to consider.

Here we apparently have a strange situation. On the one hand all scientific theories appear to be value-laden and intellectual honesty demands that we explicitly acknowledge these norms. On the other hand when we engage in scientific debates the same demand of intellectual honesty requires that we do not appeal to norms that our opponents do not share in order to support our theories. I.e. intellectual honesty appears to demand that we both acknowledge that our theories are not value-neutral and, at the same time, requires that scientific debates be conducted without appeal to such values. Surely one of the fundamental requisites of any theory of scientific rationality must be to reconcile both these demands on intellectual honesty.

It should be clear from the above discussion that any understanding of the role that norms play in science must be able to adequately reconcile the sociological rationality claim with the internal rationality claim. It should allow us to fulfil the twin demands on intellectual honesty - namely allow scientific debates to be conducted without appeal to idiosyncratic values whilst conceding that scientific theories are value-laden. The issues raised are far too complex and intertwined with many others in the theory of knowledge to make possible a complete answer to these demands until other tools in our epistemological arsenal have been developed. Nevertheless, in this chapter I shall attempt to furnish a partial answer. A more adequate response will have to await our return to the problem of the sociology of knowledge in a subsequent chapter.

2.2 Content-Free and Content-Determining Norms

Let us examine some of the kinds of norms that philosophers have considered as being involved in science. To facilitate discussion in the future I have catalogued them into different groups.

1. Traditional Norms: These are the values that were acknowledged in classical epistemologies. They include simplicity, generality, explanatory adequacy, predictive power and coherence.
2. Metaphysical Norms: These are based on metaphysical beliefs. They may demand that theories be mechanistic, phenomenalist or deterministic, or that only theories that are field theories or that exclude mentalistic terms are acceptable.
3. Religious Norms: These are often, though not necessarily, linked to metaphysical values. They are values that could exclude certain theories because they are materialistic or do not conform to the scriptures, or include certain theories because they are indeterministic and allow scope for freedom etc. Such values are not often explicitly formulated.

even if they are implicitly held.¹¹

4. Social Norms: Political, economic and social institutions, and the institutions that control and disseminate scientific knowledge (e.g. periodicals, journals etc.) all have institutionalized values that affect the kinds of theories they support; the kinds of research they fund; or the areas into which scientific activity is directed.¹²
5. Individual Psychohistorical Norms: A scientist's past experience, his competence in certain areas and techniques; his present acquaintances and interests, all affect not only the kind of problems he chooses to deal with but also the kinds of theories he considers for development.¹³
6. Aesthetic Norms: These are values like harmony, unity, beauty, simplicity that a scientist may use to compare theories.¹⁴

The list above is not meant to be exhaustive of the classes of norms that can be involved in science. Furthermore, these classes are not exclusive. Simplicity, for example, is listed both as a traditional and an aesthetic norm. Religious norms could coincide with metaphysical norms; or psychohistorical norms could also be social norms. However, the list should enable us to realize that the number of norms that influence the evolution and nature of science are very large - indefinitely large. Any one of the classes of norms (with the possible exception of the traditional norms) could contain an indefinitely large number of individual norms.

To deal with all of these norms in a more adequate manner I would like to reclassify them in a different way. In the first place we can distinguish between cognitive and noncognitive norms. The noncognitive norms are those that determine the kind of problems scientists select for study; the kind of areas that get funded for development by social and political institutions; or the cultural factors that affect or retard the development of scientific theorizing in different

situations.¹⁵ Such noncognitive norms do not dictate the kind of content theories must possess to be deemed acceptable. They merely dictate the kinds of areas of research that are deemed acceptable or important.) E.g. military interests prompted the development of ballistics science in the 17th century and, hence, contributed to the development of more adequate theories of mechanics but they did not dictate the content of such theories. In the same way political institutions today may fund nuclear energy research but not research into solar energy. However such directives do not involve specifying the content of nuclear theories or theories of astrophysics.

However, there may be cases where research itself is directed in such a way as to specify the content of a theory. E.g. an institution may fund only those who are interested in developing a Lamarckian (or Darwinian) theory of evolution. This would mean that the institution not only has research directing norms (i.e. that one should understand evolution), but also is involved in specifying the content of evolutionary theories. Insofar as it is involved in doing this it also furnishes scientists with cognitive norms.

In this chapter I shall only be concerned with dealing with cognitive norms. Cognitive norms can be divided into two classes - content-free norms and content-determining norms. Furthermore content-determining norms can be used either to specify what has to be included, or indicate what has to be excluded, from the content of a theory. Let us consider each of these types of norms in greater detail.

1. Content-Specifying Norms are those that specify, to some extent, the kinds of content a theory must have to be considered for acceptance.

An example of a content-specifying norm is the Cartesian norm that the only acceptable theories in physics are those which are mechanistic. Such a norm prescribes the content of an acceptable theory. It does this, however, only partially. The same domain of phenomena could be explained by many different sorts of mechanical theories and, as far as that norm is concerned, all of them equally satisfy it. A field theory, however, would immediately be deemed unacceptable if one subscribed to this norm. Other content-specifying norms could state that only phenomenalistic theories are acceptable; or that the only acceptable economic theory is one that assumes that labour (or capital) creates value. In fact there could even be a conjunction of content-specifying norms that a theory may have to satisfy to be deemed acceptable. E.g. one may demand that only mechanical, corpuscular, central-force based and deterministic theories are to be considered. Given any theory one would then check to see that these norms are satisfied by the theory before considering it for acceptance. Of course one may reject the theory on other grounds - it may turn out to be empirically inadequate.

2. Content-Excluding Norms are those that specify the kind of content a theory must not have to be considered for acceptance.

Mach's veto on the use of 'unobservables' in the theories of physics is an example of a content-excluding norm. It did not tell him anything positive about the content of a theory that was acceptable. It only allowed him to reject atomistic and ether theories because they postulated 'unobservable' entities. J. B. Watson's rejection of all psychological theories that used mentalistic concepts is another example of the use of content-excluding norms. Again it is possible to have a

number of content-excluding norms simultaneously operating when we decide to accept or reject a theory. However, unlike content-specifying values, content-excluding values are negative. They tell us what sort of theories to avoid, but not what sort of theories to look for.

3. Content-Free Norms are norms that, given two theories, specify which of these theories is to be preferred. They do this without either specifying or excluding directly the content of either theory. They are comparative norms.

Examples of content-free norms are simplicity, predictive power, coherence, unity, explanatory power and so on. They include all the traditional and aesthetic norms that we have listed. By merely examining a theory we would not be able to determine whether it satisfied these norms. A theory is simple relative only to another theory; it can be said to have more predictive power only by contrast with another theory. Even coherence is a comparative value because an empirical theory is never ever totally coherent (unlike a pure mathematical system), and its acceptability can only be decided by comparing the coherence achieved by other theories. For the same reason harmony and unity are also comparative values - a theory has more harmony relative to another theory; or more unity. Hence, content-free norms are comparative values.

Thus there is a basic difference between the content-free values and the content-determining (i.e. content-specifying and content-excluding) values. Content-free values are comparative; they cannot be applied to a theory on its own. Content-determining values can be applied to single theories. This is not to say that one cannot specify a measure for the predictive power of a theory on its own; or recognize

a theory to possess a certain amount of harmony or simplicity of itself. However, these values are applied to theories only in relation to other theories, i.e., to compare them against the predictive power, harmony or simplicity of a competitor theory.

We shall soon see, contrary to fairly universal beliefs, that the absolute predictive power, explanatory power or generality of a theory are not epistemically crucial values in comparing theories. What is important is the predictive or explanatory scope of a theory that does not lie within the scope of the competing theory. Furthermore, simplicity is not an important value because of the role it plays in theories as explanatory schemas, but because of the function it has when we use theories as observational instruments. We shall also find that, even if they appear to be purely prescriptive, content-determining norms also have to be construed as descriptive. For this reason we have to suppose that, like observational and theoretical statements, content-determining norms are also expectations. By recognizing that content-determining norms are expectations, we can fulfil, to an extent, the twin demands required of intellectual honesty, and reconcile the sociological and internalist rationality claims.

2.3 Content-Determining Norms as Metaexpectations

Consider the content-specifying norm that the only acceptable physical theory is a mechanical theory. It is prescriptive because it makes the demand that a theory should be considered for acceptance only if it is a mechanical theory. Yet implicit in the demand is the supposition that an adequate mechanical theory can be found to represent reality. I.e. there is a claim (even if implicit) that the relevant

domain of phenomena can be represented by a mechanical theory. If this claim were not implicitly assumed the demand would not be made. Hence, in demanding that the acceptable physical theory should be a mechanical theory, it is also claimed that a mechanical theory can represent reality. Hence, the content-specifying norm is really a demand/claim that a mechanical theory represents reality. It is an expectation that reality can be represented by a mechanical theory. This content-specifying value involves an expectation about reality - namely the expectation that reality is a mechanical system.

All content-specifying values are, similarly, expectations about reality. To say that only field theories or phenomenistic theories are acceptable is not only to demand that reality should be represented as a field or in terms of sense-data complexes; it is also to claim that reality can be represented by fields, or that reality can be represented by sense-data complexes. Being expectations, content-specifying norms are not purely prescriptive. They are prescriptive/descriptive statements that are no different in nature from the propositions involved in theories or observation reports.

Let us now consider the content-excluding norms. Take the Watsonian value that one should exclude all mentalistic concepts from psychological theories. This may appear to be purely a demand that any psychological theory should not contain mentalistic concepts. However, implicit in this demand there is also the supposition that reality does not contain any such entity or process as a mental event. Without this supposition - which is a claim about reality - the demand would collapse. Hence, the demand to exclude mentalistic concepts also implicitly contains the claim that there are no mental events. It is

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really a demand/claim that there are no mental events in nature. It is an expectation about what is not included in reality. This itself is an expectation about reality. Generalizing we can say that content-excluding values are also expectations about reality. Unlike the expectations of content-specifying values which tell us how to represent reality, content-excluding values tell us how not to represent reality.

Summarizing we can say that content-determining norms are expectations about reality in the same way that laws, theories, and observation reports are expectations about reality. Just as these other expectations are corrigible, and could be found to be inadequate to represent reality, so are the expectations embodied in content-specifying and content-excluding norms. They are as open to debate and testing as observation reports and theories are. They are not purely prescriptive statements; they are also descriptive. Hence, in the subsequent discussion I shall refer to them as metaexpectations to indicate that they are really our deepest expectations about reality.

The role of presuppositions in the development of science has only recently attained wide recognition on the part of philosophers. This has largely been due to the influence of Kuhn's book "The Structure of Scientific Revolutions". In his work Kuhn emphasizes the central role played by shared presuppositions in the development of scientific theories. Such presuppositions are offered by what Kuhn calls paradigms and, except during certain transitional periods, normal scientific practice involves solving problems and puzzles within a framework of presuppositions offered by a universally accepted paradigm. The view that presuppositions are constitutive of all knowledge had been suggested by Kant. However, for Kant such presuppositions were a priori

and therefore immutable. He conceived them to be eternal truths. Kuhn's presuppositions, however, are corrigible and scientific revolutions are precisely those episodes that involve the replacement of one set of presuppositions by another.

The idea that all knowledge is founded upon presuppositions, and that such presuppositions can be changed, was already familiar to the neo-Kantian at the end of the 19th century. It constituted an important component of Poincare's conventionalist philosophy. A similar position was held by Collingwood who believed that every proposition is meaningful by virtue of the fact that it is an answer to some question.

However, every question contains some proposition as a presupposition. E.g. to ask for the kind of particle that has traversed an ionisation chamber is to presuppose that a particle has crossed the chamber. To avoid infinite regress of questions and answers Collingwood assumes that there are some presuppositions that are not answers to any questions. He calls these 'absolute presuppositions', and these constitute for him the foundational basis upon which knowledge is erected. However, Collingwood considers such absolute presuppositions to be historically conditioned - they belong to a particular era and society and change in the course of history. Different eras in the history of science are characterized by different networks of presuppositions.¹⁶

Brown has argued that there is a need to combine Collingwood's notion of changing presuppositions with Kant's notion of presuppositions that are constitutive of all experience, to give an adequate analysis of Kuhn's view that all knowledge is directed by paradigmatically conditioned presuppositions. He further writes:-

Perhaps the most striking feature of propositions which express presuppositions is that they do not fit into the customary dichotomy between analytical and empirical propositions.

He argues against their analyticity on three important grounds. In the first place, they are not tautologies because there is no sense in which the predicate constitutes a defining characteristic of the subject. E.g. The principle that nature obeys deterministic laws is not analytic for if something could be shown not to obey deterministic laws we cannot classify it as an event lying outside of nature. Secondly, no counter-instance is possible for analytic propositions. This is clearly not the case for a presupposition like nature obeys deterministic laws. This presupposition of classical physics has indeed been denied by the quantum theory. Finally, presuppositions have to be defended by an empirically oriented research project. Such an approach would be unnecessary if the presuppositions were analytic. E.g. The proposition that nature obeyed deterministic laws was fundamental to classical mechanics and it was finally accepted only after it has shown itself to be fruitful in dealing with a large range of natural phenomena.

However, Brown also argues that such presuppositions cannot be deemed to be purely synthetic or empirical. This is because they are protected from straightforward empirical refutation. He writes:-

...paradigmatic propositions...constitute an epistemically distinct class in that they do not fit the traditional division of all propositions into a priori and empirical. Rather they are propositions which are accepted as a result of scientific experience but which come to have a constitutive role in the structure of scientific thought. At various times propositions such as all celestial motions are circular, that physical space is Euclidean, that every event has a cause, or an entire panoply of modern conservation principles have achieved this status. Many of these have been taken to be necessary, eternal, a priori truths, but some of them have nonetheless been abandoned, and it should by now be clear that every scientific proposition is subject to possible revision.¹⁸

What is interesting about the presuppositions referred to by Collingwood, Kuhn and Brown is that they are precisely those content determining norms which we have discovered to be metaexpectations. Such metaexpectations being demand/claims about how reality is to be represented cannot be deemed to be purely analytic or purely synthetic. However, Brown wants to characterize them as neither analytic nor synthetic. He does not see them as both analytic and synthetic at the same time. This is, in fact, what they are. For any such presupposition can be considered to be both an analytic definition of its terms and an empirical claim about the world.

In the last chapter we saw that observation reports and theoretical statements are expectations. The difference between fundamental presuppositions and the expectations that are observation reports or theoretical statements is that presuppositional metaexpectations being extremely general are far less amenable to direct empirical testing. They can only be tested through the theories that are articulated upon their basis. I shall consider the problem of how they are tested empirically in more detail in a subsequent chapter.

2.4 On Metaexpectations

Let us now examine the way these metaexpectations arise in science, the role they play and the reasons for their mutability. This will illustrate more clearly the claim that they are really expectations about reality. I shall begin by considering how these metaexpectations arise in science.

Metaexpectations are derived from various sources. One of the most important reservoir of metaexpectations is the corpus of highly tested

in the 18th century were hindered by the attempt to deal with chemical phenomena by assuming that they were due to particles that attracted and repelled one another on the basis of central forces.

The success of the evolutionary model in biology in the middle of the 19th century lead to a highly successful attempt to deal with other phenomena from an evolutionary perspective. Today such approaches are used in astronomy, psychology, sociology and even linguistics and philosophy. That all things are in the process of evolution has become one of the deepest metaexpectations of our culture.

Another important way in which metaexpectations arise is from philosophies and world views. These world views may be those of the community in general, or alternative world views that have been developed by counter-communities within the larger community. Because of what they are world views and philosophies often embody expectations about reality that sweep across fairly large domains of experience (if not all of experience). These expectations, precisely because of their general nature, may possess great explanatory scope (even if they are not adequately concretized to constitute a scientific theory). Beginning with such expectations could also be a powerful heuristic for generating scientific theories.

The individual experience of scientists could also furnish metaexpectations. The metaexpectations obtained from the experiences of the individual scientist tend to be idiosyncratic. However, they cannot always be considered to be as dependable or powerful as those general expectations in a world view or philosophy. These general expectations are often held precisely because many people have found them to be credible in some sense in accounting for their experience. However, the

scientific theories. These theories embody a large array of well-founded expectations about reality. By studying these theories it is possible to abstract expectations that are shared by those theories that are the most well-founded. Such expectations can be considered extremely dependable since they have shown themselves to be successful in dealing with phenomena in different domains. Hence, when we approach the phenomena in a new domain - that in some sense is considered to share features in common with the domains already studied - it can be extremely useful and time-saving to begin with such a corpus of well-tested expectations.

For example, electromagnetic phenomena, mechanical phenomena and thermodynamics, as well as chemical phenomena were known to obey the law of conservation of energy in the 19th century. Hence even when developing the theories of relativity and quantum mechanics many scientists felt it was safe to keep this law because it had been tested in so many areas of physics. It was therefore a dependable basis upon which to search for new theories. Of course, it may be said that the law of conservation of energy is not a metaexpectation, but this is only because the metaexpectations we often invoke have a simpler structure. After all, this law did function as a metaexpectation for the energeticist school led by Ostwald at the end of the 19th century.

Similarly physiological theories in the 18th century assumed that living organisms could be interpreted as mechanical systems. Though this viewpoint can be traced back to the Cartesian world view a great deal of faith in such an approach was founded on the success of the mechanical interpretation in dealing with astronomical and physical phenomena. Such an approach can also be misleading. Chemical theories

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individual's idiosyncratic metaexpectations may embody highly charged motives which could provide a powerful incentive to generate theories in which they are vindicated.

Thus metaexpectations, even if some of them are idiosyncratic, can be generated by a process of abstraction from a corpus of highly successful theories, or given by a world view or philosophy. The process of abstraction involves isolating that nucleus of expectations which are most universal within the corpus of theories or world views. Such expectations can then become the core expectations upon which a theory may be constructed.

Before constituting a network of core expectations, however, these metaexpectations have to be rendered consistent and reified. The process of reification begins with these expectations as suppositions, and then articulates them into particular theories. After reification these expectations are no longer open to question within the tradition they create. For example, out of Newton's theories of gravitation, mechanics and optics, a core of expectations were isolated and reified to form the 18th century neo-Newtonian tradition. This tradition supposed that all phenomena could be explained in terms of corpuscular interaction, central forces and mechanical determinism. Within the tradition these expectations were not open to question. The expectations that are thus reified are treated within the tradition as if they were categorical demands. In this sense they function prescriptively within the tradition. However, were the tradition to collide with an alternative one that had different expectations, it cannot treat its core expectations as only prescriptive. They have to be recognized for what they are - deeply entrenched metaexpectations of the tradition.

The reified metaexpectations become constitutive of all future expectations. This is to say that theory construction proceeds on the supposition that these metaexpectations are 'true'. The metaexpectations become part of the framework - indeed the core framework - upon which theories are constructed. Such a process can be described as the internalization of the metaexpectations into theories, and the metaexpectations are thus made constitutive of theories.

Metaexpectations have extremely significant and multipurpose roles within the tradition of science. In the first place, they can play a powerful heuristic role in guiding the creation of theories. Since they specify, to an extent, the kind of reality a scientist confronts, they guide him in selecting hypotheses to represent that reality. Given a domain of phenomena there are always a multitude of ways of creating theories that can represent it. The content-determining metaexpectations constitute very general expectations about the theory that is expected to be ultimately successful. They provide an initial framework that can be systematically enriched so that the theory becomes sufficiently concretized to confront reality directly.

They also enable a scientist to filter out theories that do not conform to the expectations about reality that he is given by these metaexpectations. The greater the number of metaexpectations, and the more specific these metaexpectations are, the more precisely they specify the kind of theories that have to be eliminated. This does not mean that the scientist has to be dogmatically committed to these metaexpectations. They merely enable a researcher to limit the class of alternative theories he has to consider in selecting the theory he wishes to pursue. He cannot, however, appeal to these metaexpectations

when he confronts an alternative theory, in scientific debate, that presupposes a different set of metaexpectations.

An important function of metaexpectations is to furnish family connections between theories. In the 19th century the success of mechanical theories of physics and chemistry suggested mechanical theories of biology and of organic evolution. It also suggested that teleological theories were unacceptable.¹⁹ Certain general expectations are common in different domains of science and could be extended to new domains. The network of connections established by these metaexpectations gives the various theories in different domains a family resemblance. It provides an interdisciplinary link and explains why advances in one domain of science can have far reaching implications for another domain. Also the multidomain applicability of these metaexpectations provides an even greater support for the adequacy of these expectations to represent reality. Isolated from all theories these metaexpectations do not constitute a scientific theory. They lack concreteness and richness of detail. They do, however, constitute a most general complex of expectations about reality. One may even go so far as to say that they specify our expectations about the structure of reality.

It should now be evident that a very important role of metaexpectations is to specify research programs. Being expectations about the structure of reality, metaexpectations like 'Reality ought to be represented by a field' or 'One ought not to recognize mental events in psychology' could be the core assumptions on which to found research programs. In practice it is not one but whole systems of metaexpectations that are involved in the specification of research programs. The

system of metaexpectations indicates the kind of theories that fall within the research program. All of the theories that are part of the research program have shared expectations about reality that would be quite different from those of another research program. We shall see later that such research programs are what Kuhn has referred to as paradigms: and paradigms can be seen to be nothing more than networks of metaexpectations.

The history of science is replete with numerous examples of how content-determining metaexpectations change with time, and with changes in the accepted scientific theories. During periods of scientific revolutions these changes can be very dramatic indeed. Metaexpectations like 'Theories should be mechanical', 'Only field theories are acceptable', 'Theories using 'unobservables' should be rejected', 'The only acceptable theory is a deterministic theory', and so on, have all been found wanting at some time or other. Indeed there are few, if any, metaexpectations that have withstood the test of time. This mutability is understandable if we view these metaexpectations as expectations about reality. As we find some of these expectations inadequate to represent reality we reject and replace them by others that we consider more adequate.

Thus, content-determining norms, though they are sometimes treated as prescriptive, are really expectations about reality. They are the most general expectations we have about reality. They are derived either by a process of abstraction from a corpus of scientific theories, or given by metaphysical, philosophical or even religious systems of belief. They could be used to specify a research tradition, or even function within one as if they were values, i.e. operate as if they were

prescriptive demands only. In a sense they are prescriptive within a tradition, because if one rejects them one is no longer operating within that tradition.

However, even if they appear prescriptive within a tradition they nevertheless remain demand/claims, or expectations, about reality. No member within the tradition would want to assert that they do not tell us something about reality. I.e they are also, in some respect, claims about reality. In paradigm conflicts where these metaexpectations collide one cannot treat them as purely prescriptive. The conflict is over incompatible expectations about reality. It is a disagreement involving different assertions about reality. This disagreement cannot be resolved by dealing with it as if it were only about prescriptive values.

It is precisely because they are expectations that they can undergo radical changes during periods of scientific revolutions. A scientific revolution reorganizes the way we cognize reality. It ends up by telling us that reality is otherwise than what it was earlier believed to be. It is, therefore, not surprising that these metaexpectations - our most general expectations about reality - are different after a scientific revolution from what they were before the revolution.

2.5 The Role of Traditional Content-Free Norms

Let us now examine the role of content-free values in the comparison of theories. It is often supposed that given two theories one should select the one to be preferred on the basis of the simplicity, predictive or explanatory power, or the generality of the theories concerned. It is of course recognized that these values need not grade the two theories in the same way. E.g. one theory may be

simpler and more general than the second, and yet have less explanatory and predictive power. It is assumed that given some sort of weighting of the significance of these values we can then decide which of the two theories is to be preferred.

However, a closer examination of actual historical episodes of admitting, eliminating and accepting theories reveals that this view is highly misleading. It shows that there have been occasions when theories have replaced others which by all measures of norms like simplicity, generality, explanatory and predictive power should have been preferable. We shall see that simplicity is not an important value merely because it helps us to select a more adequate explanatory theory - it is important because of the role it plays in using theories as observational instruments. Furthermore, in selecting theories what is important in measuring their success is their scope of prediction or explanation outside the scope of success of their competitors. It is not the actual scope of a theory but its scope that is not included in that of its competitors - and this need bear no relation to the actual scope of the theories being compared - that is crucial in their epistemic evaluation. This is what I shall call the explanatory indispensability of a theory in relation to its competitor and, in a subsequent chapter, I shall develop a measure of this notion that will enable us to rationally compare and evaluate different scientific theories.

Let us begin by examining the value of simplicity: There have been numerous attempts to characterize simplicity without much success. In certain situations this is easily done. E.g. a linear equation is simpler than a higher order polynomial; a theory that makes certain

assumptions is simpler than another that makes these assumptions and more; a theory that involves postulating numerous entities is not as simple as one that postulates a few. However, how do we compare theories that are distinct from one another in the types of entities they postulate, and in the form of their mathematical equations? Is a theory that involves a fifteenth order polynomial simpler than one that involves a sine function that can only be specified by an infinite order polynomial function? Is a theory like Einstein's that involves non-Euclidean geometries less simple than Newton's theory; or conversely is Einstein's theory simpler because it starts with fewer unobservables and is more general? In fact the problem is so intractable that Mario Bunge has characterized simplicity as a myth.²⁰

Let us take some examples of the actual application of the value of simplicity to understand how it works.²¹ It is often said, given a number of points on a graph, we use a straight line to represent these points (where we can) because it is the simplest function connecting them. Without this rule of simplicity scientists would have to join the points one by one getting a jagged line (or not join them at all, but leave them as they are, if they are particularly fastidious). Hence, without an appeal to simplicity, it is claimed, we could not even deduce that a particular relationship, say between current and voltage, is a linear one.

Is this really how scientists operate? I want to argue that the claim is misleading. What actually happens in practice is that a best straight line is drawn, and then an error factor is specified that indicates the extent to which the linear function can be trusted. It is said that the relationship is linear, $\pm 3\%$ say. There is no presupposi-

tion involved that the relationship is actually linear - only that it is representable by a function that can be specified by giving the linear function and the possible experimental error. The specification of this error is an integral part of science. Hence, if someone wishes to claim that the actual relationship is a curve that is slightly nonlinear he is allowed to do so provided his curve lies within the range allowed by the experimental error. Hence, the linearity of the curve is not an epistemic claim - simplicity merely gives us the aesthetically preferred curve, but our epistemic claim is that the relationship is specified by a curve of any kind that may lie in the range given by the linear curve and the error factor. One cannot appeal to simplicity for epistemic support in making the claim that the curve is linear.

Take the case of Newton's inverse square law of gravitation. It was considered particularly simple because it corresponded to the way light energy density diminished with distance from a point source. Therefore it appeared to possess some kind of naturalness. However, at no time did scientists as a group claim that the law was actually inverse square. In fact there was the problem of determining how closely the claim could be empirically tested. No one denied the convenience or the aesthetic preferability of the inverse square law, but neither did most scientists deny that the law was only experimentally and theoretically inverse square within an error factor. The actual law was $G \frac{m_1 m_2}{r^2 \epsilon}$ where ϵ specified the error factor. Considerable effort was spent in experimentally reducing the error factor.²²

If simplicity were such an important epistemic factor would it not have been invoked to say that there is no point in reducing the error factor further? The next simplest law would be an inverse first order

or an inverse cube law. Since the error factor is known to be small these laws do not hold. Therefore, simplicity dictates that we ought to accept the inverse square law. If it had been found that the law was actually 2.01 ± 0.003 would scientists have appealed to simplicity to claim that it should be an inverse square law? Surely the answer would have to be no. This means that simplicity may be an aesthetic value, but it is not an epistemic value. When the motion of Uranus did not appear to conform to the inverse square law there were those who were seriously prepared to modify it.²³ It was only the discovery of Neptune that put a stop to such attempts. Furthermore, Einstein's theory of gravitation did change the law in such a way that it ceased to be inverse square, and this deviation was used to account for the precession of the perihelion of the planet Mercury.

In the same way, if a theory T_1 were to postulate more entities than another theory T_2 , this would not be grounds for preferring T_2 to T_1 . Surely the only ground for considering T_2 to be epistemically preferred would be that it could predict or explain observations that T_1 could not. If T_1 can do this in some domain that T_2 cannot then T_1 has to be considered even if T_2 is preferred for reasons of simplicity. If T_1 and T_2 can deal with different aspects of the phenomena in what is considered to be the same domain (e.g. the shell model and liquid drop model in nuclear physics) then both theories have to be taken seriously. Simplicity cannot be invoked to accept or reject theories. It is a criterion that can be used to state one's aesthetic preference - not to support epistemic claims.

It has been argued that Einstein's theory of relativity was simpler than Lorentz's because it postulated fewer entities. It did not, for

one thing, require the entity of the ether. Koslow,²⁴ however, has argued against this interpretation since this removal of the ether concept was achieved only by introducing a greater complexity in predicates. Similarly, the theory that there were electrical fluids was rejected only at the price of introducing a new predicate for matter - the electric charge. Furthermore, it is not easy to compare theories with different ontologies where ontological economy cannot be decided by merely examining if one theory contains entities that the other does not.²⁵ Not only is this so in many cases when a theory is in competition with another, but advances in science often occur precisely through the introduction of new kinds of entities like genes, quasars, neutrinos or chemical elements.²⁶

Thus the situation may be described as follows. Once one has epistemic reasons to prefer a theory T_2 to a theory T_1 , one may also say that T_2 is preferable to T_1 because it is simpler. One can also say T_1 is simpler and yet claim that one epistemically prefers T_2 to T_1 . Ideally, of course, we would like the epistemically preferred theory to also be the simpler. I.e. we would like the theory we select for epistemic reasons to be also the one we would select for this aesthetic reason. For example we can desire that the cars and houses we select for good functional reasons are also the ones we prefer for aesthetic reasons. But this does not mean that we fail to distinguish the aesthetic dimension from the functional dimension. Similarly in the case of theories the aesthetic dimension of choice must not be confused with the epistemic one.

Nevertheless, one cannot deny that simplicity plays a crucial role in the selection of scientific theories. Why has this aesthetic value

been deemed so important in the history of science? The traditional answer has been that it allows for economy of conception. A theory that is simpler involves less effort to learn, to conceive and to utilize. This has been emphasized, for example, by Mach:-

"The communication of scientific knowledge always involves description, that is, a mimetic reproduction of facts in thought, the object of which is to replace and save the trouble of new experience. Again to save the labour of instruction and of acquisition, concise, abridged description is sought."

However, I want to suggest that there is an even more important reason for preferring simpler theories. These theories are ones that not only offer economy of conception but, related to this, they also allow for easier visualization. A great deal of our thinking is not conceptual but imagistic. It is easier to employ visual imagery with some theories than others. Thus a straight line is easier to visualize than a line defined by higher order polynomials; an inverse square law is easier to visualize in terms of the way light intensity diminishes with distance than a law that deviates slightly from it. A theory that postulates fewer entities is also easier to visualize than one that postulates more. Furthermore, simplicity is related to past experience: electrodynamic equations became easier to visualize once they were described in terms of hydrodynamic flow metaphors.

The reason that visualizable theories are more acceptable than others is that, given the gestalt view of perception, we would like to employ our most successful explanatory theories as observational theories through which we can increase our experience of the world. A complex explanatory theory does not easily lend itself to this role. Thus more than the desire for conceptual economy is involved in requiring that theories be simple: it is the hope that when shown to be

acceptable they can also be used as powerful observational instruments. The aesthetic value of simplicity is not as important for conception as it is for perception. Its economy ensures that it can be easily employed as a perceptual instrument after it has been tested and found to be an acceptable explanatory theory.

The concept of predictive power of a theory involves three different sorts of notions. A theory T_1 could be considered to have greater predictive power than a theory T_2 for any one of the following reasons:-²⁸

1. T_1 makes successful predictions over a larger domain of experience than T_2 ;
2. T_1 makes more precise predictions than T_2 ; and
3. T_1 makes more novel predictions than T_2 .

Hence the domain of successful predictions, the precision of the predictions, and the novelty of the predictions may all be used to measure the predictive power of theories. These are independent criteria. A theory T_1 may be superior to a theory T_2 as measured by two of these criteria, and yet not perform as well as T_2 when measured by the third criterion. For example, the heliocentric theory of Copernicus did not possess any greater predictive accuracy, or embrace a larger domain of phenomena than the Ptolemaic theory. Yet it predicted many novel phenomena that the older theory could not (one example is the phases of Venus). Plack's hypothesis, in 1900, did not predict any novel phenomena nor did it embrace a larger domain than the wave theory. Nevertheless, it has greater predictive precision in dealing with the problem of blackbody radiation. At the end of the 19th century both thermodynamics and statistical mechanics were equally successful in

generating novel phenomena and in their accuracy of prediction (though not for the same sort of phenomena), but thermodynamics embraced a much larger domain (e.g., chemistry and radiation phenomena) than statistical mechanics.

The above examples make it clear that one can judge the predictive power of a theory along various dimensions. It may be possible to have a function that assigns to a theory a predictive power either as a total predictive power or a separate predictive power along each of the dimensions considered. However, even so the history of science shows that the predictive power of a theory is not a value that appears to be involved in the epistemological evaluation of scientific theories.

To illustrate the above claim, consider a theory T_1 that predicts with great accuracy over a large domain D_1 and is fruitful enough to generate a proliferation of novel predictions in this domain. However, suppose that there is another theory T_2 that makes more successful predictions within a small subdomain D_2 of D_1 . Let us assume, however, that T_2 's predictive precision in D_2 is not as great as T_1 's predictive precision in the rest of D_1 outside of D_2 - then the overall predictive accuracy of T_1 would be greater than T_2 . Furthermore, assume that T_2 predicts fewer novel phenomena than T_1 . Then T_1 would clearly be superior to T_2 in terms of the extent of its domain of successful predictions, its accuracy of prediction in its successful domain, and the number of novel phenomena it predicts. By any one of these criteria taken individually, or all of them together, the predictive power of T_1 would be greater than that of T_2 .

The question is would the scientific community reject T_2 and only accept T_1 ? Historical examples show that as long as a theory can make

more precise predictions than any other theory within some domain, or so long as it can make novel predictions that no other theory can, it would be considered an admissible theory for that domain and for those phenomena it predicts. Thus what is important is not the absolute predictive power of a theory but its ability to predict that is not included in the scope of its competitor. In having a predictive scope that lies outside the scope of success of its rival, the theory shows itself to be indispensable for predicting some phenomena. It is this indispensability (relative to its competitor) that makes the theory admissible.

Einstein's photon hypothesis of 1905 is a case in point. As compared to the wave theory its domain of success was smaller. Though more accurate within the domain it addressed itself to, it cannot be deemed more accurate than the wave theory outside this domain. Furthermore, though it predicted novel phenomena one could hardly claim that, in its history, the wave theory was any less prolific in generating novelty. Hence the only reason that Einstein's theory was considered admissible was that it was able to successfully predict where the other theory could not - not because, over and above this ability to predict, it also had something called greater predictive power. By doing this the theory revealed that it was indispensable in making certain predictions.

What is relevant in science is to ask whether a theory predicts successfully where another cannot, but not whether a theory has greater predictive power than another. Nuclear physics provides another illustration of this point. Here we have numerous models that apply to the same domain of phenomena. Some are able to predict certain kinds of phenomena, and others are needed to predict other kinds of phenomena, in

the nuclear domain. Each of these models is useful in dealing with some dimension of nuclear phenomena. It cannot be assumed that all of these are not admissible theories because they are mutually incompatible. Nor is any measure of their total predictive power going to cause scientists to reject some model so long as it can successfully predict some range of phenomena that other theories cannot. The only time a theoretical model can be rejected is when it is only able to predict phenomena that other theories can also predict, and where the domain of its predictions is included in other theories. In this case the theory is no longer indispensable to make predictions because it has no scope outside that of its competitors. Any theory that can predict novel phenomena that other theories cannot, or can predict with greater precision, would be scientifically admissible.

Like predictive power, the notion of explanatory power can be construed in many ways. A theory T_1 may be considered to have greater explanatory power than a theory T_2 for one of the following reasons:-

1. T_1 explains a larger domain of phenomena than T_2
2. T_1 explains the phenomena with greater precision than T_2

Both the above senses of explanatory power are very closely related to the notions (1) and (2) of predictive power that we have discussed.

They can be shown to be not epistemically crucial measures for the same sort of reasons that were given for the corresponding predictive powers.

However, there are also other senses in which a theory T_1 may be said to have greater explanatory power than another theory T_2 :-

3. T_1 explains the phenomena with fewer assumptions than T_2 ; and
4. T_1 explains the phenomena by supposing assumptions that are more compatible with the other major accepted theories of

science than T_2

Let us take the assertion that a theory T_1 that makes fewer assumptions than a theory T_2 must, for that reason, be epistemically preferred to T_2 . It is often not easy to see what is meant by fewer assumptions. If T_1 makes certain assumptions, and T_2 makes these assumptions as well as others, then it is easy to see that T_1 is to be preferred. In this case, however, we are not saying that T_1 is preferred to T_2 because it makes fewer assumptions, but because the extra assumptions made by T_2 are not necessary to explain or predict the phenomena. These assumptions are thereby dispensable. This is a trivial case and can hardly be considered to offer any great insight into the problem.

Another alternative is to axiomatize the theories T_1 and T_2 and count the number of axioms each requires. This does not seem to be a fruitful approach. In the first place many important theories of science have resisted any sort of axiomatization: e.g., Darwin's theory of evolution, Hebb's theory of the central nervous system or Freud's theories. In fact there are some philosophers who have questioned the possibility of fruitfully axiomatizing most of the theories of science.²⁹ Hence counting axioms is probably a procedure that would work only in the case of a restricted class of theories - e.g., classical and relativistic mechanics.

Furthermore any set of axioms can be reduced to a single axiom by conjunction. This can, of course, be avoided by some sort of ad hoc device, but we are nevertheless left with the option of giving the same theory different axiomatic formulations. These axiomatic formulations would be equivalent, but there is no need for them to assume the same number of postulates.³⁰

Even if we do manage to axiomatize two theories that are intended to apply to the same domain of phenomena, it is not obvious that counting axioms would be an adequate method of dealing with the problem. For any particular theory is used to explain phenomena, not only on its own, but in conjunction with a network of auxiliary theories. These auxiliary theories contribute as much to the explanatory adequacy of the theory in dealing with different sorts of phenomena as the theory itself. E.g. the success of the quantum theory in explaining atomic spectra depends not only on the quantum theory, but also on the theory of relativity and the theories of the structure of the atom. Similarly the Lorentz electron theory attempted to explain spectroscopic observations using classical mechanics and classical electrodynamics. It would be reasonable to assume that when we compare the Lorentz theory with the quantum theory, as to their ability to explain spectra, we must not only take into account the assumptions involved in these theories, but also those involved in their auxiliary theories. However, if we were to do this we would not be counting only the number of axioms involved in each of these theories. We would instead be comparing the axioms of networks of theories against each other. Hence, it is difficult to see how one can characterize the explanatory power of a theory by counting the axioms that are implicitly involved in using the theory.

The above point can be further illustrated by considering other historical examples. It has often been maintained that the Ptolemaic theory can be made observationally equivalent to the Copernican theory provided we are prepared to add a sufficient number of epicycles. Therefore the only reason for preferring the heliocentric theory has to be attributed to the economy of assumptions involved in it. This,

however, is a misleading claim. For the performance of these theories depends not only on the theories themselves, but also on the auxiliary theories used in conjunction with them. Thus one of the strong arguments for the Newtonian synthesis was its prediction of the return of Halley's comet that was made by using the Copernican theory in conjunction with his theory of gravitation. Such a prediction would have been impossible within the Ptolemaic framework.

The reason why philosophers of science are able to make claims of the sort that a theory T_1 is equivalent to another theory T_2 and that, therefore, some sort of economy of conception must have lead scientists to prefer T_2 to T_1 , is their tendency to isolate these theories from the rest of the conceptual framework in which they are embedded; and to limit the discussion to a restricted domain of phenomena. Thus by restricting the phenomena concerned to planetary motions only, it can be certainly argued that the Ptolemaic system (enriched with epicycles) and the Copernican system are equivalent. However they cease to be equivalent the moment we are prepared to deal with the whole domain of phenomena with which these theories are concerned, and especially, if we consider the sorts of predictions possible for these theories when they are used in conjunction with their auxiliary theories.

Similarly it has been argued that Maxwell's theory with certain additional assumptions is equivalent to continental action-at-a-distance theories; or that Einstein's special theory is equivalent to Lorentz's theory with the clock-retardation hypothesis. This, however, is plausible only if we ignore the predictions possible on the basis of these theories when they are used in conjunction with other auxiliary theories. For Maxwell's theory can be used to give an account of optics

in conjunction with the hypothesis that light is an electromagnetic wave since it allows for the presence of energy in empty space. No action-at-a-distance theory can conceivably do this. Also Lorentz's theory is not compatible with the quantum hypothesis of Planck, but Einstein's special theory was used in conjunction with the quantum theory in a highly successful way by Sommerville to predict the fine lines of the hydrogen atom, and Dirac to formulate his relativistic quantum theory. Hence, whatever formal equivalence is found to exist between these theories, it can be seen to be spurious if we take into account the fact that the success of the theory depends on the sort of predictions it is able to make in conjunction with other auxiliary theories. Even where two theories are able to deal with phenomena equally adequately the fact that one theory involves more assumptions is not grounds for rejecting it. Shrodinger's wave-mechanical quantum theory is equivalent to Heisenberg's matrix mechanics except for the fact that it postulates the continuity of the wave function. Heisenburg's theory does not require this additional assumption. Nevertheless this has not been considered sufficient grounds for rejecting one theory and preferring it to the other.

Consider now the claim that a theory has greater explanatory power if it does not make assumptions that conflict with other major theories of science. It is precisely those theories that are most revolutionary, and give us the greatest insight into reality, that would be deemed inferior by this requirement. Planck's quantum theory, Darwin's evolutionary theory and Kepler's theory all made assumptions that were incompatible with most of the assumptions made by other major theories of their time. It is this that made them so significant.

Generality is a concept that applies to the domain of a theory. A theory T_1 may be said to be more general than a theory T_2 if:-

1. The domain of success of T_1 is larger than the domain of success of T_2 ; or
2. The domain of potential applicability of T_1 is larger than the domain of potential applicability of T_2 .

Logical empiricist philosophers require that successor theories should always be more general than their predecessors on both these counts.

Even a philosopher like Lakatos, who is critical of the logical empiricist tradition, requires that a new theory can be progressive with respect to an earlier one only if these two conditions are fulfilled.

In the first place, the theory must have excess empirical content over its predecessor to be theoretically progressive. Secondly, some of this excess content must be corroborated for it to be empirically progressive. A theory constitutes what he calls, a progressive problemshift only if it is both empirically and theoretically progressive. This is to require that a theory have both its successful and potential domains larger than its predecessor to be considered progressive.³¹ Such a view is similar to Popper's position, and has also been adopted by Zahar.³²

Clearly these two criteria are independent of one another. Given two theories, T_1 and T_2 , one may find that T_1 is successful over a larger domain than T_2 , and yet consider that T_2 has the potential to be successful over a larger domain than T_1 . Ideally, the best theory is one that is general in both senses.

Is generality an epistemic criterion? Given two competing theories T_1 and T_2 let us assume that the domain of success of T_1 is larger than that of T_2 . Assume that T_2 succeeds in a small domain where T_1 does not

and, furthermore, T_2 's potential domain is smaller than T_1 's. Does this mean that T_2 could be rejected by appealing to the value of generality? Einstein's photon hypothesis can be used to make the same point again. So long as a theory shows its indispensability to predict and/or explain a given range of phenomena, however small the domain, one cannot appeal to the value of generality to reject it.

There is only one situation where an appeal to generality can be successful - this is where the domain of success of one theory T_1 includes the domain of success of another theory T_2 . In this case it is not generality, but dispensability, that causes us to reject T_2 . Since T_1 accounts for everything that T_2 can, the latter becomes a redundant theory because it has no scope of success outside that of its rival. However, if there is even a small domain of phenomena that can only be successfully dealt with by T_2 , T_2 will not be jettisoned. There would, of course, be numerous attempts to develop the more general theory T_1 so that it is able to deal with these phenomena. However, there is no guarantee that this would be successful.

Furthermore, when a general theory is replaced by a new theory, the successful and potential domains of the new theory need not include the corresponding domains of the earlier theory. Take the case of Lorentz's electron theory. This theory dealt with the problems connected with the electrodynamics of moving bodies, atomic spectra and the constitution of matter. It was ultimately replaced by the theory of relativity, which dealt with the problems of the electrodynamics of moving bodies; the quantum theory, which dealt with atomic spectra; and the theories of atomic structure, which dealt with the constitution of matter. None of these theories could be said to include the domain of the Lorentz theory.

in their domain. Nevertheless, they replaced the Lorentz theory by subdividing its domain between them. In this case we cannot say that any of these theories are better than the Lorentz theory because its domain of success included the domain of success of the Lorentz theory.³³

The reason these theories replaced the Lorentz theory was because each of them was able to predict and explain phenomena more successfully in those areas it had appropriated from the Lorentzian domain. If an overall generality is the significant criterion used to account for the success of the new theories, it is difficult to see how any one of them can be said to be more general than the Lorentz theory. This is because their domains only partially overlap that of the Lorentz theory.

Therefore, it seems to me that even if we can compare theories in terms of their generality, this cannot be used as an epistemic criterion to accept or admit theories. A theory that is less general than another theory may nevertheless be admitted for a particular domain of phenomena if it can predict and explain the phenomena more adequately in this domain. Even if the other theory is found to be more general, the theory that is successful in the smaller domain cannot be considered eliminated.

An analysis of the history of science reveals that this is a fairly common occurrence. Even though the Ptolemaic and Copernican theories explained astronomical phenomena, the Copernican theory could not account for why bodies fell to the earth in a natural way. There was no such problem for the Ptolemaic theory because the earth, being at the center of the universe, it was natural to suppose that bodies were drawn towards it. To deal with the phenomena within the Copernican framework we require an auxiliary theory of gravitation. Similarly, the caloric

theory could give an account of heat that explained convection, conduction and radiation as due to the transfer of a common substance - the caloric. The kinetic theory of heat, on the other hand, could not deal with the problem of radiation. Instead, the transfer of heat by radiation had to be explained by ether theories that were used in conjunction with the kinetic theory.³⁴

The reason why theories cannot be compared as to their overall generality is that theories themselves are involved in specifying their domains. There is no reason to suppose that when one theory replaces another, the latter has to classify events in the same way as the former. Events considered to belong to the same domain by one theory may be relegated to different domains by successor theories. This reclassification of events means that we cannot require that the events explained by one theory must necessarily be subsumed under the domain of a single successor theory. Instead, the domain may be divided into subdomains each of which is dealt with by a different successor theory. In this case it is difficult to see how generality can be used to compare two different theories.

We may, therefore, conclude that the actual generality, predictive or explanatory power of theories is not involved in their epistemic evaluation. Nevertheless, there is an important intuition they attempt to capture - namely, that some sense of scope is relevant to theory comparison. What is important is the scope of a theory that lies outside the scope of its rival. This scope bears no necessary connection to the actual scope of the theory (though it usually increases with the scope of the theory).

2.6 Scientific Norms and the Externalist-Internalist Debate

We have seen that the cognitive norms that philosophers have considered to play a role in science can be divided into two classes-- content-free and content-determining norms. Contrary to what has been traditionally maintained content-free norms like explanatory and predictive power and generality, are not epistemic. Also, contrary to these views the content-determining norms have been found to be neither prescriptive nor descriptive. They are metaexpectations. As such they are (and must be) open to criticism the way all expectations are. One cannot appeal to an expectation one has about reality in order to support that same expectation about reality. Therefore, such metaexpectations are empirically corrigible and mutable.

These expectations, however, can be far more general than theories and, therefore, be harder to criticize. Systems of such expectations could constitute world views, research programs or philosophical positions. However, insofar as they lead to theories that can be tested, and insofar as they are required to be consistent, systems of expectations like these are also corrigible. This is because new expectations are being constantly introduced into the system as observation reports. Even if these observation reports are theory-laden, they also depend on a reality independent of our expectations. In this way contradictions could arise in a system of expectations that requires us to revise our field of expectations.

Let us now reconsider the epistemological problems that were raised earlier in this chapter to see if this alternative approach to the question of norms casts some light on them. Take the controversy between the internalist and the externalist accounts of scientific

rationality. This difference has also been characterized as sociological versus internal, or historicist versus logicist. (I am not sure that the distinctions made by adopting these various labels make the same distinction. However, it is generally true that those who make the sociological claim are also historicist and externalist; and those who make the internalist claim are also logicist. There are, of course, those who deny that such a distinction can be made.)

The externalists claim that sociocultural values are involved even in the context of evaluation of theories. The internalists deny that this is the case. They claim that, even if a scientist is influenced by social factors in the context of creating theories, such factors play no role in the context of evaluation of theories. Both positions could be tenable if we interpret these norms as metaexpectations. The externalist will then be correct in surmising that such metaexpectations are involved in the context of evaluation of theories; and the internalist will be correct in affirming that these sociocultural metaexpectations cannot be treated as purely prescriptive in the evaluative context. Thus the internalist can accept that there are socially induced metaexpectations (after all he does affirm these expectations are internalized into theories in the context of creation), but he could rightfully assert that they cannot be used as prescriptive norms to reject theories that are founded on different metaexpectations. This is because metaexpectations are as much expectations about reality as are the other statements of a theory.

The externalists have claimed that differences between paradigms cannot be resolved, because they also involve norms. These norms being prescriptive no factual data can affect them. However, since the norms

they refer to are metaexpectations their claim is untenable. Expectations cannot be dealt with as if they were purely prescriptive. It is true that they have a prescriptive dimension to them, but it is equally true that they also have a descriptive dimension. Hence, they could be rendered incompatible with observation.

Thus by giving up the view that there are any purely content-determining prescriptive norms involved in the epistemic evaluation of theories we can reconcile the externalist and the internalist positions. What the externalist is affirming is that sociocultural metaexpectations are involved in the context of evaluation of theories. What the internalist is affirming is that the fact that these metaexpectations are socioculturally derived is not relevant in the context of evaluation theories. Both of these claims are tenable. The externalist is claiming that the kind of theories that get developed, and the kind of metaexpectations that are involved in getting developed sufficiently to participate in scientific debate, are affected by sociocultural factors. The internalist could affirm this. Yet he could claim that, insofar as these theories enter the context of scientific debate, they are evaluated solely on their ability to deal with the domain of phenomena they seek to explain.

However, this reconciliation is effected only by requiring a sacrifice on the part of the externalist and the internalist. The externalist cannot claim that what he refers to as norms are purely prescriptive. The internalist cannot claim that the theories that get developed sufficiently to participate in scientific debates would have been the same theories even if the sociocultural milieu had been different.

There are sociocultural influences on the kind of expectations that are

articulated into theories, or that directly enter the context of a scientific debate; but the fact that these expectations are socioculturally engendered, or engineered, is irrelevant to their epistemic evaluation.

We can, therefore, say that the internalist is correct in supposing that there can be a rational reconstruction of the internal history of science. The externalist is also correct in supposing that there could be a significant sociological account of the history of science that explains why one sort of scientific theory was replaced by another. They are two perspectives on the same history. They are not incompatible perspectives.

This account enables us to fulfil the twin demands on intellectual honesty that was mentioned at the beginning. We saw, that to be honest, we have to recognize that our theories are value-laden, as well as acknowledge that we cannot appeal to values that our opponents do not share in order to support our theories. If we replace values by metaexpectations it is easily seen that this can be done. We can recognize that our theories involve metaexpectations and, at the same time, acknowledge that we cannot appeal to metaexpectations that our opponents do not accept in order to support our theories. If we do appeal, it can only be to metaexpectations that they are also prepared to allow.

2.7 Conclusion

In this chapter we have seen that scientific norms which are content-determining have to be construed as metaexpectations. Such metaexpectations play an important role in science - they constitute some of the deepest presuppositions of research traditions. We could

even say that they offer a prescriptive ontology - an apparent contradiction for classical empiricists which is resolved once we recognize that the prescriptive form also implicitly embodies a descriptive claim. Those of the positivist tradition have often frowned upon such meta-expectations - they were considered metaphysical and a great deal of energy has been expended by logical empiricists to purge science of them.

We have also found that norms like the predictive power, explanatory power or generality of theories are not crucial to their epistemic evaluation. What is of significance is the scope that theories possess outside the scope of their competitors. This insight will be utilized in a subsequent chapter to develop a more adequate measure of theory evaluation - one that allows us to determine when a theory is admissible, acceptable and preferable to other theories. Such determinations are central to any theory of scientific rationality. The measure I propose will explain many features in the history of science that cannot be accommodated so long as we take the absolute scope of a theory to be crucial in its epistemic evaluation.

CHAPTER THREE

THE HOLARCHIC THEORY OF MEANING

3.1 The Traditional View and Its Problems


Theories of meaning constitute one of the cornerstones of any epistemology. Speaking loosely, one may say that these fall into two classes - atomic and holistic theories. Atomic theories of meaning tend to presuppose that terminological meaning can be given in isolated bits so that what a term means can be specified more or less independently of the conceptual framework in which it is embedded. Such a term may be given meaning either by empirically associating it with elements of experience given independently of other conceptual assumptions; or by explicitly defining it in relation to other previously defined terms. In contrast, holistic theories consider that the meaning of a term cannot be understood without also specifying the theoretical context in which it occurs. A scientific term is a knot in a conceptual web; the strands in the web are the propositions of a theory, and the meaning of the term is given by its location relative to other terms. The total structure of the web affects the meaning of each of the terms contained in it.

It is generally the case that those who subscribe to a predominantly atomic empiricist view of knowledge also adopt atomic theories of meaning. This is the case with philosophers like, Russell, Carnap and Nagel. Contrariwise, those sympathetic to holistic empiricist positions are also inclined to support holistic theories of meaning.¹ Clearly this is a highly schematic account for, as we shall, see atomic

empiricists like Nagel also recognize holistic meaning for some terms; and even a holistic empiricist like Quine seems to tend towards atomic views of meaning for certain terms. This is not surprising, because, as we shall see, neither an atomic nor a holistic account of meaning is adequate. But a satisfactory theory of meaning cannot be achieved in the way that Nagel explicitly and Quine reluctantly attempt to do - i.e. by separating some terms that are given meaning atomically and others that acquire meaning in a holistic manner. We have to develop a holarchic theory in which all terms are seen to possess both an atomic and a holistic dimension of meaning. In order to develop the holarchic theory I shall consider the limitations of one important version of traditional atomic theories of meaning - the version offered by the logical empiricists who in some sense are the culmination of the atomic empiricist tradition.

The logical empiricist philosophy, in its sophisticated variant, essentially involves a dual language model of science. On the one hand, it assumes that there is a language of observation which involves using terms that are defined by directly associating them with elements of experience. These observation terms were originally conceived to refer to sense-data by Carnap, but insuperable difficulties in carrying through this phenomenalistic program led him, and others, to modify their position so as to accept a language of physical objects.²

Nonetheless, even in the modified program, observation terms can be given meaning either by empirical association, or by ostensive or operational definitions in which they are correlated to what is directly given to experience.



On the other hand, science also involves the use of terms that are theoretical, and which cannot be defined by an appeal to sensory experience. Theoretical terms, for the logical empiricists, are those like 'atoms', 'ether' or 'electric field' which have to be defined, not by association with experience, but in relation to other observation terms. They hold that this can be done by the use of explicit definitions; by postulation of the properties of the entities referred to by these terms; or by partially interpreting theoretical terms in relation to observation terms. Such procedures are held to reduce the meaning of theoretical terms to those of observational terms. This implies that, unlike observation terms which obtain their meaning directly from experience, the meaning of theoretical terms is ultimately parasitic on the meaning of observation terms.³

This dual language model is essentially an attempt on the part of the logical empiricists to elaborate and develop Hume's views on the nature of cognitively significant discourse. Hume had divided all perceptions of the mind into two distinct categories that he called impressions and ideas.⁴ For Hume impressions are the immediate objects of awareness given in experience - colours, tastes, smells, feelings and so on. Ideas are those perceptions that arise when we imagine, remember or reflect. Ideas are of two sorts - simple and complex. Simple ideas are direct copies of impressions except that they are less forceful and lively. By combining simple ideas in various ways we can form complex ideas. The imagination can create complex ideas but not simple ideas. Thus the range of ideas that can be created by imagination is limited by the stock of impressions that have been experienced.

Hume couples this view to his theory of meaning. According to him meaningful propositions are of two kinds - matters of fact and relations of ideas. Statements that involve matters of fact refer to experience and their truth can only be established by reference to experience. Statements giving relations of ideas assert connections between ideas and their truth is determined solely by reflecting upon these ideas. Translated into modern terminology, matters of fact are empirical statements and relations of ideas express the connection between meanings of terms. Statements that express neither matters of fact nor relations of ideas are, for Hume, meaningless propositions.

The logical empiricist philosophy fundamentally involves an elaboration of Hume's view. Firstly, to stretch the Humean metaphor, it made scientific theoretical discourse significant only if it were possible to establish a relation of ideas between the terms of a theory; and secondly, observational discourse was significant only if it was about what was directly given to experience, and the terms of the discourse were ultimately defined by associating them with elements of experience. Thus the empirical significance - or meaningfulness - of theoretical discourse was achieved derivatively by its relation to the observational language; and the empirical significance of the observational language is established by connecting its terms directly to experience.

One of the most comprehensive accounts of this dual language model of science is that offered by Nagel.⁵ Consider the example of Euclidean geometry. According to Nagel pure Euclidean geometry can be formulated as an abstract calculus by giving a set of postulates in which the expressions 'point', 'line', 'plane', 'congruent with', 'lies between' and several others are used as basic terms. These expressions have

connotations that are associated with familiar spatial experiences, but such connotations have to be suppressed or ignored in the deductive elaboration of the postulates. Within the postulational treatment of geometry points and lines have to be considered as 'things that satisfy the conditions stated in the postulates'. They have, therefore, to be taken to be implicitly defined by the postulates in which they are embedded. However, for the theory to explain experimental results, the terms of the calculus have to be linked with experience. This is done by means of correspondence rules. One way of doing this is to provide rules that link the notion of a 'straight line' to the path of light rays and that of a 'point' to the intersection of such rays. The moment this is done it becomes possible to interpret the theorems of the geometry as empirical assertions about the world. In the process we have also provided a visualizable model of the abstract calculus. Thus Nagel's account distinguishes three components in a theory. Firstly there is a logical skeleton given by an abstract calculus which implicitly defines the basic terms of the system. Secondly these terms are given empirical meaning by correspondence rules that link them to observation terms. Finally a model of the abstract calculus is offered which allows us to interpret the theory in a familiar conceptual or visualizable form.

Consider another example. The kinetic theory of gasses contains terms like 'molecule' and 'kinetic energy of molecules'. These terms cannot be defined directly by appealing to experience. They are instead defined by the postulates of the theory which specify what are the properties that have to be satisfied by anything that can be called a 'molecule' or referred to as the 'kinetic energy of molecules'. Thus the

postulates implicitly define these expressions.⁶ This abstract calculus can be given a model by viewing molecules as a large number of small elastic spheres that are in constant motion and engaging in collision with themselves and the walls of the containing-vessel. To link the theory to experience we require correspondence rules. There is no correspondence rule that allows us to link the instantaneous kinetic energy of a molecule to experience. However, there is a rule for the average kinetic energy of molecules. This rule states that the average kinetic energy of the gas molecules is proportional to the absolute temperature of the gas. By using this rule it is possible to explain the macroscopic behaviour of the gas in terms of the microscopic behaviour of the molecules.

There are a number of features of Nagel's atomic empiricist account that should be noted. These would become increasingly significant in the light of the theory of meaning I shall subsequently develop. In the first place he assumes that it is possible to make a sharp distinction between theoretical and observational terms. We have already seen that the theory ladenness of experience makes such a distinction untenable. Of course Nagel recognizes that it may not be possible to give a rigorously precise sense of observable in practice.⁷ This, however, does not preclude him from assuming that the distinction is possible in principle.

Secondly, he employs two distinct methods to specify the meanings of terms - the method of implicit definitions and that of empirical associations. Observation terms are supposed to obtain their meaning by associating them with empirical states-of-affairs either by means of ostensive definitions, operational definitions or experimental procedures.

Theoretical terms, on the other hand, are given meaning of a result of being implicitly defined by the connections established between them by the postulates of the theory. Of course, these connections do not give them any empirical significance - this is provided by the stipulation of correspondence rules. However, correspondence rules are not explicit definitions of theoretical notions - Nagel doubts that such explicit definitions are possible.⁸

Thus the distinction between observational and theoretical terms is intimately connected to the distinctive ways in which their meaning is given. For observational terms it involves the use of what we may call empirical associations; for theoretical terms it is the method of implicit definition. This means that Nagel assumes a holistic theory of meaning for theoretical terms, and an atomic theory for observational terms. Any change in the postulates of the theory would affect the meaning of the theoretical terms contained in it, but observational terms will not be affected since they acquire meaning only through empirical associations.⁹

Nagel's atomic conception of meaning - albeit for observation terms - faithfully adheres to Hume's atomic conception of experience. The basic elements of experience are impressions, and language associates linguistic signs to these independently given units of experience. This association of sign to impression can be given independently of the rest of our conceptual framework and is the foundation upon which we erect theories. This is also the basic presupposition of all other atomic empiricists who have followed Hume. Thus for the earlier Wittgenstein facts constitute the basic units of experience; the world is the totality of facts; elementary (atomic) propositions assert the existence

of a state-of-affairs; and no elementary proposition can contradict another. Theories are superstructures that are tested against elementary propositions that offer the facts.¹⁰

The dual language model is incoherent because its basic presupposition, that there are some terms which are given meaning by direct empirical associations and others which acquire meaning by being embedded in a theoretical context, is false. All terms are theoretical because all observation is theory-laden. Furthermore, nearly all terms in science also possess meaning by empirical associations - even those that are apparently highly theoretical.

Take Carnap's distinction between theoretical and observational terms. He assumes that the latter refer to what are directly observable. By directly observable Carnap meant that the truth of a sentence in which only observation terms occur can be determined by relatively few observations and the use of simple instruments.¹¹ However, Achinstein has argued that such a definition is extremely vague and ambiguous, and leaves us with many unanswered questions.¹² Is a term to be considered theoretical only if no aspect of what it refers to is observable without the use of instruments? In this case typical theoretical terms for Carnap, like temperature, kinetic energy, charge, mass and entropy, would have to be considered observable. For changes in the referents of each of these terms can be detected even without the use of any instruments.

Secondly, there are many terms that refer to observables that can only be detected by the use of sophisticated instruments. We can observe cell nuclei only under microscopes; and telescopes are required to see the moons of Saturn. Nonetheless, we cannot consider cell nuclei

or Saturnian moons to be theoretical entities. Thus the use of instruments cannot be a criterion to demarcate theoretical from observational terms.

Neither can we say that a term is theoretical only if instruments are generally required to measure it. In this case terms like volume and weight would have to be classified as theoretical. Thus to make the distinction on the basis of instruments would, according to Achinstein, require us to classify as observational what Carnap would typically consider to be theoretical terms and vice versa.

Achinstein also criticizes the view that the number of observations required to apply to a term provides a criterion to separate observational and theoretical terms. A physicist, for example, may be trained to identify an alpha-particle emission in a cloud chamber by a few observations of the track patterns. However, to determine if a colour term like 'dark ultramarine' applies to an object may require making a larger number and a more carefully controlled set of observations. Nevertheless, the expression 'dark ultramarine' is hardly more theoretical than the expression 'alpha particle emission'. Thus, neither the use of instruments nor the number of observations required to apply a term provides an adequate criterion to demarcate between observational and theoretical terms.

Putnam¹³ presents an even more serious argument against the tenability of the distinction. He shows that any observational term can always be used to refer to unobservables. Thus Newton used the term 'red', which is clearly observational in Carnap's sense, to talk about red light corpuscles. Thus, we cannot draw the distinction between observational and theoretical terms by asserting that the former can in

principle only be used to refer to observables. The same point has been made by Hesse who argues that the distinction is untenable because the term 'spherical' is an observable when applied to baseballs but not protons; or the term 'charge' is observable with respect to pith balls but not ions. Thus, depending on the context, even those predicates that could be deemed to be typical cases of ~~observable~~ terms could be made to apply to unobservables.

Similarly, even what are traditionally considered theoretical terms could be made to be observable in a new context. Thus the theoretical term 'gene' becomes observable when genes are identified with DNA molecules in a micrograph. Similarly the ratio of charge to mass of an elementary particle can be read off the geometry of its tracks in a magnetic field. In each of these cases making the theoretical term observable was not merely a matter of developing new instruments; it required theoretical knowledge to know that genes are DNA molecules, or that track geometry could be used to measure mass.¹⁴ Similarly, though a proton may be observed in an ionisation chamber, it was theoretical knowledge that made it possible to know that what was observed was a proton.

In the traditional logical empiricist view observation terms are those whose meanings are given independently of any theory by empirical associations. This view holds that since these elements of experience are independent of our theories the meanings of observation terms are immune to revision as a consequence of theoretical change. We have seen that even if sense-data are given our experience is not. Sense-data are only indicators of external events; experience is constituted by perceptual gestalts that are organized on the basis of theoretical

knowledge around sense-data. Empirical associations are made to gestalt elements of experience which are theory-laden. Thus theoretical change may require us to revise them. Even a predicate like 'red' could be withdrawn from a receding star on the grounds that it is not the colour of the star but a result of the Doppler effect.

Neither can we say that theoretical terms are unlike observational terms because they cannot have empirical associations. As we saw earlier, terms like proton, temperature and electric field can all be applied in situations that involve empirical associations. Changes in temperature can be felt; an electric shock is a good indicator of the presence of an electric field; and elementary particles can be identified visually by means of tracks they make in bubble or cloud chambers.

By far the strongest objection to the distinction between theoretical and observational terms arises from the theory-ladenness thesis. The observational theories that are used to look at the world structure our experience of it. Even if sense-data are given, the gestalt units of experience into which they are organized are not. In attaching signs to aspects of experience - i.e. in the instituting of our linguistic conventions - we already presuppose a theoretical framework. For this reason none of our scientific terms can be theory-free. All terms, even those that we acquire when we first learn language, are theory-impregnated.

One of the fundamental motives that many atomic empiricists have in making the sharp distinction between theoretical and observation terms is that, it is felt, the latter can be used to formulate observation statements that would provide a foundational basis for science. Since

all observation reports are expectations they cannot furnish such an incorrigible basis. Given the problems connected with the distinction it would be far better to do away with it altogether.

Thus let us relinquish, once and for all, the distinction between observational and theoretical terms in science. There are only terms which may be given meaning either by empirical associations to experience, or by implicitly defining them in relation to other terms. All terms may be given meaning by both of these methods. The observational meaning of a term is given by its empirical associations; its theoretical meaning by the background of statements in which it is embedded. All terms have both theoretical and observational meaning, but the terms themselves are neither theoretical nor observational.¹⁵

3.2 The Holarchic Theory of Meaning

This view enables us to understand and to circumvent many of the objections confronted by the traditional distinction between theoretical and observational terms. Take the assertion that naked eye observability cannot be the criterion for distinguishing between theoretical and observational terms, since cell nuclei are observable only through microscopes though they cannot be considered to be theoretical entities. The above view enables us to say that cell nuclei are observable, insofar as they can be given meaning by empirical association, even if this has to be done with the aid of an instrument like the microscope. For the same reason lunar craters are observable given the telescope; protons are observable in ionisation chambers and genes are visible under the electron micrograph. Scientific instruments are extensions of our senses, and there is no reason to suppose that the empirical associa-

tions they furnish cannot be said to give observational meaning to terms.

It has also been pointed out that observational terms are affected by the conceptual framework in which they are embedded. This causes no difficulty to our present view because we suppose that all terms are implicitly defined by the conceptual framework. Thus what were traditionally conceived to be terms defined only by empirical association can now be seen to also possess theoretical meaning. Similarly all terms that were traditionally considered theoretical could also possess meaning by empirical associations. Terms like proton, charge, mass, temperature and entropy all possess observational meaning because we can sense directly what they refer to.

The view that terms have both theoretical and observational meaning does not require us to relinquish the special status afforded to scientific observation reports in contrast to the other statements that occur in science. For it is only in the case of observation reports that we actually employ the observational meanings of terms to arrive at a scientific statement. Observation reports are direct responses to experience. Thus an observation report like 'Whales suckle their young', is different from a theoretical prediction 'Whales suckle their young', based upon the theory that whales are mammals. The former is made by employing the observational meaning of the terms involved; the latter by using the theoretical meaning of the sentences involved in deducing it. In collapsing the distinction between observational and theoretical terms we have not collapsed the distinction between observation reports and the other statements of science.

Another way of looking at the matter is to say that a term has observational meaning if it can be used to make observation reports. In this case it is not obvious that any scientific term can ever be said to lack observational meaning. Take the term 'atom' in the nineteenth century when there were no electron microscopes that could render individual atoms visible the way they are now. Even at that time it would have been possible for someone to use atomic language to make observation reports. He could say 'The atoms of the gas have moved further apart' instead of 'The gas has expanded'; or 'I see that the mean kinetic energy of the atoms has increased' instead of 'I see that the temperature of the gas has increased'. Even the most theoretical of scientific terms can be used observationally. What is to prevent a person from reporting 'The quantum waves of two light beams are interfering' instead of 'I observe an interference pattern on the screen'?

The recognition that all terms of science have both observational and theoretical meaning suggests that we cannot presuppose that what is observable is necessarily what is real. All observation occurs in the context of a theory and the fact that a phenomenon is observed does not guarantee its veridicality. Even counterfactuals can sometimes be directly observable. Those who saw the sun rise in the east for thousands of years were taken in by an illusion. One may observe an oasis that is only a mirage. There are numerous examples in perceptual psychology of illusions that deceive our senses. Churchland¹⁶ has provided an interesting way of actually learning to see the planets in the heavens through the Copernican framework so that we have an experience of them as orbiting the sun rather than the earth. He also

illustrates that even the language of the defunct caloric theory may be used as an observational language. Thus an attempt, like Mach's, to purge science of terms like 'ether' or 'atom' because they are unobservable is misdirected; so is the attempt to develop an instrumentalist view on the basis that some terms are purely conceptual instruments (or theoretical).

The denial of the connection between the observable and the real is not grounds for alarm or despair: it does not undermine the empirical basis of science. What is cognized to exist is determined by our true theories of the world, and what is observed in the world through our true theories is cognized to both exist and be observable. The theory-ladenness of observation does not mean, as the holistic empiricists mistakenly suppose, that there is nothing in experience that is not given independent of theory. We have noted that sense-data are such givens. But the theory-ladenness of observation does mean that sense-data are not all that is given to experience. This is where many atomic empiricists have been misled. Instead of viewing sense-data as indicators to be used to experience the world, they have presumed that this is all there is to the experience of the world. A gestalt empiricist view avoids both these alternatives: what we experience are gestalt constructs based on sense-data. These gestalt units of experience are directly observable, and would give what can be deemed to exist, if the theory used to construct them is also true. Such a gestalt experience is neither purely constructed nor purely discovered. The world of our experience is both invented and found; created and received.

This suggests a view of science that is dramatically different from traditional accounts, and will become increasingly evident as we develop

our ideas. The standard view of the atomic empiricists supposes that scientific theories are erected upon a foundational experience that is given for all time. Instead, we have to assume that, given a network of theories to observe the world, we have a certain gestalt experience of it. We can then give new terms observational meaning by empirical associations to such gestalt units of experience. These terms may be used to formulate observation reports, that could lead us to new theories. However, successful new theories may require us to modify some of our earlier theories - and thereby reject some of the ways in which we had construed experience. This would also lead us to reject earlier observation reports. The growth of science thus involves a bootstrapping process rather than a cumulative one - observational theories are used to furnish experience in order to support new explanatory theories; these new theories may subsequently require us to modify some of our previous observational theories. Also the new explanatory theories may themselves be employed as observational theories to furnish different experiences of the world. Theories do not grow upon the bedrock of experience; our theories and our experience evolve together and mutually determine one another.

The distinction between theoretical and observational terms bears a connection in traditional philosophies to the distinction made between analytic and synthetic statements. Giving up the former also requires us to relinquish the latter. In the same way that the analytic/synthetic distinction is projected by the traditional view onto separate classes of statements, when it actually expresses the complementary aspects of every statement, the theoretical/observational dichotomy is also projected onto separate classes of terms when, in

fact, it expresses the two different ways in which all scientific terms are given meaning. That there are two modes of specifying the meanings of terms is not grounds for supposing that there are two classes of scientific terms.

We have seen that the atomic empiricists tend to emphasize the observational meaning of terms, and to play down the role of its theoretical meaning. This is why they expend so much labour in the attempt to relate theoretical terms to the observational vocabulary. In contrast the holistic empiricists, like Kuhn and Feyerabend, tend to exaggerate the role of theoretical meaning. At times they seem to suggest that theoretical meaning is all there is to the meaning of terms.¹⁷

It is important to recognize that, even though theoretical and observational meanings have to be distinguished, they cannot be totally separated. For even in the case of giving meaning through empirical associations we have to presuppose the theories through which our experience is obtained. It is the observational theories we employ that furnish the perceptual gestalts which are referred to in defining observational meaning. However, once these observational theories are assumed the observational meaning of terms can be given atomistically. Changes in the rest of our conceptual framework will not affect observational meanings, provided we keep our observational theories fixed. Theoretical meanings, however, are extremely sensitive to any conceptual changes, since these are given by implicit definition through the conceptual network of propositions in which terms are embedded. Thus, observational meanings are only dependent on that part of the conceptual framework we employ as observational theories (and even then,

only partially, because the experience is also affected by what there is in the external world); theoretical meanings are dependent on our conceptual framework as a whole. In this sense observational meanings have relative autonomy with respect to our conceptual framework, but not theoretical meanings. Thus, the meaning of a term can be deemed to be neither completely atomic nor completely holistic - it is holarchic.

The concept of a holon, and a holarchy, was first introduced by Koestler in connection with biological systems and then generalized. He writes:-

"To get away from the traditional misuse of the words 'whole' and 'part' ... I proposed some years ago, a new term to designate those Janus-faced entities on the intermediate levels of any hierarchy which can be described either as wholes or as parts, depending on the way we look at them... We have seen that biological holons, from organisms down to organelles, are self-regulating entities which manifest both the independent property of wholes and the dependent properties of parts. This is the first of the general characteristics of all types of holarchies..."

As Koestler indicates the parts of an organism cannot be considered to be completely dependent upon its relations to other parts, or to be completely independent of such relations. They are both semi-autonomous entities and constituent parts of the larger system. They are holons, and the system itself is a holarchy. Similarly, a society is a holarchy of individuals - individual behaviour is both autonomously determined in some respects and socially determined in others.

Though I do not wish to maintain that the terms of language satisfy all the requirements prescribed by Koestler for holons, we may nevertheless use the extreme suggestibility of the concept to define terms as holons. Their function, by virtue of their observational meaning, is relatively independent of the conceptual framework, since

observational meaning is affected only by that part of the conceptual framework we have selected in order to organize perceptual gestalts. Their function, by virtue of their theoretical meaning, is intimately affected by the whole conceptual framework. Thus, scientific language involves terms linked in a holarchy, and the total meaning of a term is determined holarchically. The theoretical component requires a holistic theory of meaning and the observational component an atomic theory of meaning.

However, even atomic observational meanings are not given independently of any conceptual presuppositions. The autonomy of observational meanings is only relative, since they are given by empirical associations to perceptual gestalts that are, themselves, theoretically determined structuring of sense-data. Nevertheless, the distinction between atomic and holistic meaning is useful, because of the importance of separating from the rest of our conceptual framework that part employed as observational theories. Where there is conceptual disagreement, without disagreement about what is given to experience, we may presume agreement about observational theories; disagreement is only about explaining what is mutually perceived. More radical disagreement involves evaluating the acceptability of observational theories. In this case the veridicality of our experience itself is in question. The deepest conceptual conflicts put into question, not only the theories we use to explain events, but also the observational theories we employ to determine what events there are in the world. We shall deal shortly with the problem of resolving such deep-rooted conflicts.

In recent years the holistic (or network) theory of meaning has been associated most conspicuously with Quine. Nevertheless, the

tension between the holistic and atomic conceptions is very evident in his work and leads to a significant ambiguity in his position.¹⁹ In his classic paper on the untenability of the analytic-synthetic distinction he writes:-

Any statement can be held to be true come what may, if we make drastic enough adjustments elsewhere in the system ... Conversely,²⁰ by the same token, no statement is immune to revision.

However, in a later work he maintains that observational sentences are incorrigible.

...the philosophical doctrine of infallibility of observation sentences is sustained under our version. For there is scope for error and dispute only insofar as the connections with experience whereby sentences are appraised are multifarious and indirect, mediated through time by theory in conflicting ways; there is none insofar as verdicts to a sentence are directly keyed to present stimulation.²¹

The reason for this vacillation on the part of Quine is evident when we distinguish between the observational and the theoretical meaning of terms. In his earlier work Quine was concerned with the sentence as a part of a system of other sentences. Hence its theoretical meaning - given by the theoretical meaning of its terms - was holistically determined by the system as a whole. Systemic considerations could lead us to revise the sentence. In the later work Quine was concerned with the stimulus (observational) meaning of the sentence and, since he proposes that this is independent of systemic connections, it was difficult for him to see how observation sentences could be corrigible.

The problem arises for Quine only because he assumes that stimulus meaning involves linking terms to theory-independent elements of experience. If we assume that what is perceived are theoretically

structured gestalts there is no reason to presume that changes in theory - or rather observational theories - cannot lead us to dramatically alter our observation reports. It is true that we often employ as observational theories only that part of our conceptual framework that has been found to be most dependable; for this reason alone observation reports are relatively stable compared to other statements.

Nevertheless this is not grounds for taking them to be infallible or considering them to be only 'keyed to present stimulation'. They are certainly responses to the stimuli offered by sense-data, but they are keyed to perceptual gestalts organized around such sense-data.

3.3 The Dynamics of Meaning Change

Recent debates in the philosophy of science have emphasized the changes that are involved in the meanings of scientific terms when scientific theories change. There are many examples to illustrate this in the history of science. The term temperature underwent a shift in meaning from the time it was first defined in terms of the expansion of mercury, then in terms of the expansion of gases followed by its definition in terms of the electrical resistance of various materials. In each case the modification in meaning was motivated by the desire to achieve a more adequate system of theories to represent various phenomena. To define temperature in terms of gaseous expansion allowed the representation of the mean kinetic energy of the molecules of a gas to be proportional to its absolute temperature.

Similar, the meaning of the terms 'length', 'time', 'field' and 'wave' have been modified in the history of physics. In quantum mechanics the term 'wave' does not represent an undulation in an

etheral medium, as it did in classical physics. The terms 'length', 'time' and 'mass' represent frame-dependent quantities in relativistic physics, whereas they were considered to be absolute frame-independent quantities in Newtonian physics. Similar changes of meaning occur in all areas of science. The terms 'mixture' and 'compound' in Dalton's chemistry do not have the same meaning as in Lavoisier's chemistry; the use of the term 'atom' by Dalton to represent indivisible particles is not the use in modern chemistry.

The holarchic theory of meaning enables us to understand in detail the dynamics of terminological meaning change. A model that accounts for such changes of meaning has been developed by Hesse and can be adapted to our point of view if we do not make the distinction between theoretical and observational terms. Hesse develops her model as follows. She begins by showing that all descriptive predicates function both by means of direct empirical associations and by means of sentences containing other descriptive predicates. Thus, the meanings of predicates are given both by reference to experience and by defining them in relation to other predicates.

Hesse then proceeds to consider how language is acquired. The first predicates are learnt only by means of empirical associations. New predicates can then be taught both by giving their meaning implicitly in terms of predicates already known, or by using empirical associations. As the learning of the language becomes sufficiently advanced some of the predicates may be used to formulate general statements or laws. And it is at this point that the possibility may arise that the meanings given to these predicates by the relations between them established by these laws may come into conflict with prior

meanings they may already have been given. Thus we may have to modify the meaning of the predicates to eliminate this inconsistency. She writes:-

...the system of predicates and their relation in laws ... become(s) sufficiently complex to allow for the possibility of internal misfits and even contradictions. This possibility arises in various ways. It may happen that some of the applications of a word in situations turn out not to satisfy the laws which are true of other applications of the word. On the other hand the range of applications may be widened in conformity with a law, so that a previously incorrect application becomes correct.

To illustrate her position she cites the following examples. The term 'element' was applied to water in Aristotelian chemistry in which earth, air, fire and water were the constituents out of which all the material in the sublunar world was constructed. (The heavenly bodies were held to be constructed out of an incorruptible fifth element or essence - the so-called quintessence.) The application of the term element to water was later withdrawn in order to preserve a system of laws which required that elements could not be dissociated into parts that are themselves elements; that elements always enter into compounds; that every substance is composed of one or more elements and so on. Similarly the term mammal that originally applied only to land animals was later applied to whales in order to conform to the law that only mammals suckle their young.

The above examples of Hesse reveal how theoretical change may cause us to modify the observational meaning we give to terms. However, changes in observational meaning may also occur because more empirical data reveal inconsistencies in its application. Thus colour predicates which were originally applied in relation to how objects appear may be restricted to apply to objects only in sunlight, after the discovery.

that their colours looked different in some artificial lighting. Red tomatoes, e.g., appear black in blue light. Similarly, time used to be measured by using water-clocks and pendulums, but inconsistencies in the readings offered by these two methods lead to abandoning the water-clock as a standard.

Changes in meaning may also be the result of attempts to deal with theoretical inconsistencies. The inconsistencies inherent in classical mechanics and Maxwell's electrodynamics lead Einstein radically to modify Newtonian mechanics. This in turn results in a dramatic change in the meanings of the most fundamental terms of science. Thus, meaning change could occur as a result of theory change, or in the attempt to deal with inconsistencies in the application of the observational and/or theoretical meanings of terms.

3.4 Theory Comparison and Background Equivocating Languages

The theory-ladenness of experience, the expectational view of statements and the holarchic theory of meaning together constitute such a radical revision in our conception of the nature of experience, language and meaning that it is no longer possible to compare scientific theories in the traditional way. This is because given two conceptual frameworks that appear to use the same vocabulary - say, the frameworks of classical and relativistic physics that use expressions like mass, length, time, force, and so on - the comparison cannot presuppose that one can appeal to a common experience to test these theories; a core of observation reports common to these two frameworks; or even a language that is shared by these theories. We cannot assume a neutral experience because all experience is obtained through the framework; we cannot

presume a neutral empirical basis because all observation reports appeal to theory-dependent experience; and we cannot presuppose a common language because the meaning of every term is dependent on the theoretical context in which it occurs. Since all the statements of a conceptual framework are also analytic definitions that implicitly define the meanings of the terms involved, each term has a theoretical meaning different from that it will possess in another framework. This would be true not only of theoretical meaning but also of observational meaning, since the latter are given by empirical associations to theory-dependent experience. Thus, those who employ different conceptual frameworks do not even share the same language - there seems to be no neutral background of shared language in which they could communicate with one another. Different conceptual frameworks seem to define their own experience, create their own empirical basis and offer different languages. It appears that they cannot be rationally compared to one another; that they offer incommensurable worlds; and that those using the language of one framework cannot communicate with those employing another.²³

Such a problem does not arise within traditional atomic empiricist views like those held by logical empiricists. Here it is assumed that one can have a pure observation language that refers to an experience of the world that is offered to us independently of any theory. All observation reports can appeal to this foundational experience and would, as a class, constitute an incorrigible and theory-free empirical basis. Furthermore, there is also a common language of communication - that of the neutral observation language. Even if the meanings of theoretical terms are incommensurable because they are defined

implicitly by the statements of the theory, this would not prevent the theories being comparable against a background empirical basis, or discourse being possible in a language of shared meanings.

Philosophers like Popper and Laudan recognize that a pure observation language is not possible, given the theoretical framework in which experience occurs. Instead, they argue that the way to create an empirical basis is to formulate all observation reports by using theories that do not presuppose the theories being compared. Thus we would have a neutral background against which theories can be tested. It is true that the reports would be theory-laden, but since they are common to the supporters of the theories in question, the background theories would provide a common language in which these theories can be compared. Thus Laudan writes:-

"...neither correspondence rules nor a theory-free observation language are necessary for comparing the empirical consequences of competing theories ... the terms in which a problem is characterized will generally depend upon the acceptance of a range of theoretical assumptions T_1, T_2, \dots, T_n . These assumptions may, or may not, constitute the theories which solve the problem. If a problem can be characterized only within the language and the framework of a theory which purports to solve it, then clearly no competing theory could be said to solve the problem. However, so long as the theoretical assumptions necessary to characterize the problem are different from the theories which attempt to solve it, then it is possible to show that the competing explanatory theories are addressing themselves to the same problem."²⁴

The appeal to background theories, however, cannot deal with the problems raised by meaning incommensurability if we consider the most basic and revolutionary theories of science. In comparing theories like classical mechanics and the theory of relativity, we have to recognize that even the most fundamental observation reports used to test them are

formulated by employing terms introduced by these theories. Terms like mass, length and time only acquire meaning by being embedded in the theories of classical or relativistic physics, and we cannot formulate any observation report to test these theories without presupposing these terms. To test and compare such theories, we do not have a deeper set of background theories that allow us to give these terms meanings that do not presuppose either a classical or a relativistic framework. Thus the solution provided by Popper and Laudan - appeal to neutral background theories - cannot be employed in comparing such theories.

How then can such theories be compared? Do we have to acknowledge that Kuhn and Feyerabend are correct - that these theories involve incommensurable conceptual worlds, that they cannot appeal to some background experience, and that they cannot be tested against some sort of background empirical basis? Such a relativistic solution is premature because a background language can be constructed from the two theories. It is neither a pure observation language nor a language formulated independently of the theories being tested. It is, what I shall call for reasons that will become obvious, an equivocating background language.

To construct this background language we proceed as follows. Suspend all the empirical associations of the terms of classical and relativistic physics. The two theories would then become uninterpreted systems of sentences. The only meanings their terms would have would be those theoretical meanings given to them as a result of being implicitly defined by the sentences in the system. Take all those sentence expressions that are common to both classical and relativistic physics. These sentence expressions implicitly define the terms in them in the

same way that the primitives of an uninterpreted geometry are defined by its postulates. Thus the theoretical meaning of its terms will be shared by both relativistic and classical physics. Give these terms only those empirical associations that are common to relativistic and classical physics. Then the observational meanings of the terms in it will also be common to both Newtonian and Einsteinian science. Let us call this language the background equivocating language - or BEL - of the theories from which it has been constructed. Since the terms of BEL have theoretical and observational meaning common to the two conceptual frameworks from which it has been constructed, the language of BEL is common to them.

The construction of BEL furnishes us the means to create a background experience common to the theories being compared, and an empirical basis that is neutral relative to them. We have seen that the problem centred on the fact that, since the two theories bring along with them different observational theories (and a part of them could also constitute the observational presuppositions), the experience furnished by one observational framework may not be acceptable to someone who proposes the alternative theory. However, if the observational theories used to compare them are expressed in BEL there is a common experience against which both relativistic and classical mechanics can be compared. And if the reports appealing to this experience are formulated in BEL then the theories have a background empirical basis.

This is only natural. BEL is a language, but it is also a network of theoretical assumptions. Given the expectational view of statements every language - or web of linguistic conventions - is also a set of

theoretical (empirical) claims. Thus if the observational theories employed only presuppose the theoretical assumptions in BEL the experience furnished through them - i.e. the way these assumptions are used to organize sense-data into perceptual gestalts - can be used to test both the theories from which BEL has been constructed. The reports made in BEL by appealing to this experience given through BEL, would provide the background empirical basis to compare the theories.

It is true that the BEL-experience is a restricted one. It excludes all the experiences possible within either relativistic or classical physics that is not possible in the other. This is only proper, since we cannot allow as evidence in comparing the two theories those experiences that are only available in one conceptual context. The acceptability of using parts of the conceptual framework in an observational context is itself under test; and the veridicality of the experience they furnish is open to question.

However, once we have shown that one of the conceptual frameworks is rationally acceptable, it is possible to also accept the whole range of experience it furnishes. We can also accept as evidence for it all the extended experience that was not possible using only BEL. We can now do this because the successful conceptual framework has proven its viability against its competitor. It did this without presupposing that its unique theoretical assumptions were involved in creating the evidence against its competitor. A theory has to prove itself before it can be used to furnish data. In this respect a theory is like any physical instrument used in observation. Take the telescope. When it was first invented it has to be tested against the results of naked eye observation. Its success in this range was what justified scientists in

accepting the information it furnished of things inaccessible to the naked eye. All scientific instruments are tested similarly - they are calibrated against independently confirmed knowledge before being employed to furnish new information.

I have referred to BEL as the background equivocating language. To perceive the significance of this description it is, perhaps, best to begin by considering conceptual systems whose terms are defined only implicitly in relation to other terms. This is the case, for example, with uninterpreted systems of geometry. Such systems have no empirical content since their terms possess no observational meaning. Pure Euclidean geometry constitutes such a system - the axioms of the geometry implicitly define its primitive terms. It becomes applied geometry only when these primitives are given observational meaning through a physical interpretation. By doing this we obtain an empirical theory.

Consider now the uninterpreted systems of hyperbolic geometry. This contains the primitives 'straight line' and 'point' which also appear in Euclidean geometry. These primitives can be considered to have theoretical meaning given to them by the axioms of the geometry. Independent of these axioms they have no meaning - they are merely signs.

Since the axioms of the geometries are different, the meanings of the terms 'straight line' and 'point' in the two systems cannot be the same. It may appear that, as a consequence, geometers cannot compare the theorems of the two systems. For example, take the sentence 'The medians of a triangle intersect at a point'. This sentence expresses a theorem of both Euclidean and hyperbolic geometry. However, can this allow us to assert that the theorem is common to both geometries? It

may appear that since a triangle and its medians are defined in terms of straight lines and points, and these primitives have different meanings in the two geometries, the meaning of the theorem cannot be the same in both systems. The two geometries cannot be said to share the same theorem about triangles. Likewise, they cannot be said to have any theorems in common. They are incommensurable systems.

I want to say that the agreement is possible, not because they agree on the total meaning of the sentence 'The medians of a triangle intersect at a point', but only on that partial meaning furnished by constructing the BEL of Euclidean and hyperbolic geometry. This background language is that given by suspending the parallel postulate for the two geometries. Then we obtain a restricted meaning for the primitives given by all axioms, except the axiom of parallels. This is what defines Saccherian geometry. Since the theorem about medians can be deduced from Saccherian geometry it is also a theorem of Euclidean and hyperbolic geometry.²⁵

The significance of Saccherian geometry is that, being the background equivocating language to Euclidean and hyperbolic geometries, all its theorems can be read as Euclidean or hyperbolic theorems. Thus theorems of Saccherian geometry are equivocating with respect to these other geometries. If someone were to write a set of sentences expressing theorems of Saccherian geometry both Euclidean and hyperbolic geometers would have to accept them. All geometers would agree that Saccherian sentences are theorems, even if the total meaning of the terms in these theorems were not shared by them.

This suggests that Saccherian geometry can provide the empirical basis, when Euclidean and a particular hyperbolic geometry are so.

interpreted as to apply to physical space. This could be done by identifying the path of a light ray as a straight-line. Then we could measure the angles of a large triangle employing only interpreted Saccherian geometry. All such measurements would not be deemed partial to Euclidean or hyperbolic geometry, and would be acceptable within either framework. If the sum of angles should turn out to be consistently smaller than two right angles, there would be good grounds for rejecting Euclidean geometry (or at least believing that the evidence does not support Euclidean geometry, since there may be alternative explanations for the discrepancy).

What is significant about such an experiment is that it does not involve theory-free procedures or observation reports. The observation is made in the context of assumptions - those involving Saccherian geometry and the expectation that light rays are in a straight line. Someone who did not share the expectation that light rays travelled in straight lines would not be prepared to accept the validity of the experimental test.

Neither is the experiment made in the context of theories that do not assume any of the theoretical claims involved in Euclidean or hyperbolic geometries. The experiment does not employ only background theories independent of the theories being compared. Most of the axioms involving Euclidean and hyperbolic geometries are accepted in Saccherian geometry. Thus the test between the two theories does not involve using a theory-free language, or a language independent of the theories being compared.

The language actually employed is one that is equivocating with respect to the theories being tested. It is equivocating because it can

be interpreted as acceptable using the framework of either theory. This is possible because Saccherian applied geometry is constructed by accepting those sentence-expressions that are common to the two geometries (when they are taken as uninterpreted systems), and giving its terms those empirical associations they share in both geometries. I.e., it was constructed to be a background equivocating language to the other geometries.

The use of theory-equivocating procedures and language is fairly general in science and, in fact, is required in any reasonable experimental design intended to compare different theories. Scientists employ a great deal of thought and effort, in the design of experiments, so as not to make tacit presuppositions that favour one theory to another. This involves a considerable amount of conceptual analysis. The Michelson-Morley experiment, experiments to measure the variation of the mass of elementary particles, the transverse Doppler effect, or the gravitational red shift all involved designing experiments that did not favour the interpretation of classical or relativistic physics. This means making observation reports and observations only in the context of the background equivocating language of these theories. When this is done the experiments used to compare classical and relativistic physics can be read in a theory-equivocating manner: by merely watching someone perform and record these experiments it would not be possible to decide whether a classical or relativistic framework was being employed. The results, however, would show that the above experiments disconfirmed the predictions of classical physics and supported those of Einstein.

The use of equivocating procedures in comparing theories is recognized by Feyerabend but his excessive holism and nonfoundationalism

preclude him from perceiving that they involve the use of an equivocating background language.

"...the Michelson-Morley experiment, the variation of the mass of elementary particles, the transverse Doppler effect are said to refute classical mechanics and to confirm relativity. The answer to this problem is not difficult either. Adopting the point of view of relativity we find the experiments, which of course will not be described in relativistic terms of length, duration, mass and speed and so on, are relevant to the theory, and also support the theory. Adopting classical mechanics (with or without the ether) we again find that the experiments which are now described in the very different terms of classical physics (i.e., roughly in the manner which Lorentz described them) are relevant, but we also find that they undermine (the conjunction of electrodynamics and) classical mechanics. Why should it be necessary to possess terminology that allows us to say that it is the same experiment which confirms one theory and refutes the other?"²⁶

Feyerabend's radical nonfoundationalism prevents him from recognizing that they are the same experiments. Given the gestalt view we need not adopt such a counterintuitive stance. The identity of the experiment is given by the identity of the sense data given to experience. What our theory does is to simply organize such sense-data into elements of gestalt. We perceive prisms, telescopes, spectra, vernier scales etc. in the Michelson-Morley experiment because we employ our background equivocating observational theories to organize the sensory indicators presented to experience. Reading these sensory indicators in an equivocating manner we recognize that the experiments support one theory and violate the predictions of the other.

The gestalt view of experience does not lead to the radical relativism inherent in holistic views that deny given sense-data. The traditional atomic empiricists had sought for a foundation in experience. Such a foundation, they assumed, had to be independent of

theories in order for objective knowledge to be possible. This was a view espoused by widely divergent thinkers such as Locke, Descartes and even Kant. The holistic empiricists deny that such a foundation is possible, but they presume that the nonexistence of a foundational experience has to necessarily imply a relativistic conception of knowledge. Both atomic and holistic empiricists assume that objective knowledge is possible only if there is some sort of foundational experience. We have seen that this is unnecessary.

Even though experience depends on language, those who adopt different conceptual frameworks can compare their theoretical presuppositions by appealing to a common background experience. This background experience is relative to both the languages being compared. It is the experience obtained through observational theories that are given in the background equivocating language of the theories being compared. This experience is not foundational, but it does provide an objective basis for comparing these conceptual frameworks.

Furthermore, the experience is not static either. It is constantly evolving as the theories being compared evolve. Being obtained through a language constructed out of the theories that are in competition, it is a situation specific experience obtained through a similarly situation specific language; one that is designed to mediate communication and agreement between those who do not share conceptual frameworks. This means that the empirical basis to compare these theories - i.e., the class of observation reports that are common to both theories (because they are formulated in BEL, and appeal to the BEL-experience) - is also contingent on the theories being compared, and evolves along with them.

Holistic relativism and atomic absolutism share a common set of presuppositions. They assume that comparisons between different world conceptions, and world perceptions, is only possible if there is an independently given common world of experience, and a shared language of meanings. The atomic empiricists presume that such a language and experience exists - hence they allow that objective knowledge is possible. The holistic empiricists deny this, and assume that the alternative is radical incommensurability or relativism. There is no reason why the problem should be seen to present a dilemma - it is possible to create out of the very world views being compared a common language and a common experience. This is precisely what BEL and the BEL-experience provide. Though depending on the theoretical frameworks being compared, they are neutral with respect to them.

It is also clear that, the more divergent two conceptual frameworks are, the more restricted would be the common language and experience they can appeal to. Both Newtonian and Einsteinian scientists can appeal to quite an extensive background equivocating language, but an Aristotelian physicist would have a much smaller language in common with a relativistic physicist. In fact, as the differences between conceptual frameworks widen they tend to move towards the language of the so-called pure sense-data. In the final analysis there may be agreement about nothing but the sense-data (provided we assume that they have the same sensory-organs). But agreement about sense-data is extremely small agreement indeed - it is to agree about the indicators that inform us about the world. It is not any agreement about the nature of the world.

The requirement that nonrelative knowledge is possible only if one can have both a foundational experience and a language of commonly

shared meanings is highly artificial. No such foundational experience is possible. Also, given the expectational view of statements and holarchic theory of meaning, no two speakers can ever give the same meaning to the terms they employ, even if they can be said to share a common general framework. For the most minor differences in the statements they accept affects the meanings of (nearly) all the terms they employ. There can be identity of meaning for two speakers only if they share all theoretical principles. This is a highly unrealistic requirement to impose for allowing any agreement between speakers. To say that world views are incommensurable because their terms have different meanings is to assert no more than this - that they are incommensurable if they are, in any way, different.

3.5 On Dialectical Reasoning

The construction of the background equivocating language illustrates the important role of dialectical reasoning in science. Though a great deal has been written on the nature of dialectics since Plato, there has been little progress in developing a dialectical logic - especially when contrasted with the achievements of deductive logic. This is partly due to the complexity of the subject; but it is also because, as frequently noted, the rules of dialectical logic are themselves subject to evolution, and crucially dependent on historical contexts. For the purposes of this discussion I want to deal with dialectical reasoning as that which is concerned with debates between scientific communities - a mode of reasoning that involves the active deployment of alternative theories whose antagonisms and agreements are used to develop one or more of them; and which is also utilized to

mediate agreement and communication between those who do not share conceptual frames of reference. This is the role of a hermeneutic dialectics as Radnitzky points out:

"Applied hermeneutics has remained closely connected with the problem of communication - both as 'co-understanding' and 'agreement' between different languages and cultures. ...Hermeneutic human sciences study the objectifications of human cultural activity (texts etc.) with a view to interpreting them, to find out the intended or at least the expressed meaning in order to establish a co-understanding or possibly even consent which has not (yet) been obtained or repairing the same - such which has been disturbed; and, in general, to mediate traditions so that the historical dialogue of mankind may be continued or resumed, and also deepened."²⁷

Holistic and atomic empiricists have generally ignored the role of dialectical modes of reasoning although, it must be acknowledged, the former have not totally ignored the significance of the interaction of incompatible alternative theories to the growth of science. They have assumed that all scientific reasoning is either inductive or deductive. The traditional hypothetico-deductive model of theory-testing presupposes that theories are compared solely by comparing consequences deduced from them against observation; or by determining the inductive support that experience offers to theories. Such an account would generate no difficulties if it were possible to have a foundational experience to test theories. The holistic empiricists recognise that there is no such foundation; hence, they conclude, all such testing is inevitably circular. Experience arises in the context of the framework it is intended to support; alternative frameworks (where they are very dissimilar) appeal to different observations and are, consequently, incommensurable and noncomparable.

The inherent difficulty presented by deductive or inductive reasoning, in comparing and testing different theoretical frameworks, is

that these methods do not allow us to deal with more than one framework at a time. Deductive logic restricts us to explicating the logical consequences of the presuppositions already internalized in the theory. These are then checked against experience; but where the observation report itself involves the assumptions of the theory, it does not allow us to transcend them. We are not offered a way of getting out of the exclusive presuppositions of each of the alternative theories so that they can be tested against an experience that could - in some way - be neutral with respect to them. Also deductive reasoning cannot offer any means of arriving at a language that could be employed to mediate agreement, or disagreement, between different theories. Meanings being internal to a theory, there is no possibility of communication between those who stand on either side of a conceptual divide. Though we can appeal to an experience given internally to a conceptual framework to test its theories, we cannot use a hypothetico-deductive approach, on its own, to compare different conceptual frameworks directly against a common experience. Confined to the method, we appear to be caught in a vicious circle where the experience used to support a framework is, itself, structured by the theories of the framework.

An inductive mode of reasoning does not solve the problem either. Inductive reasoning appeals to experience, and any such approach has to presuppose the observational theories involved in achieving the experience. The degree of inductive support a theory has - given an empirical basis - depends on the degree of support we are prepared to accord these observational theories. Like hypothetico-deductive reasoning any model of inductive reasoning also faces a vicious circularity - the experience that has to be appealed to, in order to

determine the empirical support for theories, is itself offered only internally to the conceptual framework being tested.²⁸

This is not to deny a role for deductive and inductive methods in scientific research. Deductive reasoning is important in revealing the structural relationships that exist between networks of statements.

Inductive methods can be employed, internal to any conceptual framework, to test the degree of support that observation furnishes to the theories in that framework. However, both these modes of reasoning do not allow us to cross frameworks. They do not show how one can use language to facilitate dialogue across frameworks; or how one can appeal to experience to mediate the empirical comparison of theories.

It is this difficulty that has led holistic empiricists like Kuhn and Feyerabend to deny that the rational comparison of radically different theories is possible. They argue that there is no language that is common to different frameworks (since meanings are dependent on theoretical contexts); there is also no shared experience to test such theories (since all perception is theory-relative). Using Galileo's revolution in physics and astronomy Feyerabend illustrates his point:-

"When the 'Pythagorean idea' of the motion of the earth was revived by Copernicus it met with difficulties which exceeded the difficulties encountered by contemporary Ptolemaic astronomy. Strictly speaking one had to regard it as refuted. Galileo, who was convinced of the Copernican view and who did not share the quite common, though by no means universal, belief in stable experience, looked for new kinds of fact which might support Copernicus and still be acceptable to all. Such facts he obtained in two different ways. First by the invention of the telescope which changed the sensory core of everyday experience and replaced it by puzzling and unexplained phenomena; and by his principle of relativity and his dynamics which changed its conceptual components. Neither the telescope phenomena nor the new idea of motion were acceptable to common sense (or to the Aristotelians). ... The whole rich reservoir of the

everyday experience and the intuition of his readers is utilized in the argument, but the facts which are invited to recall are arranged in a new way, approximations are made, known effects are omitted, so that a new kind of experience arises, manufactured almost out of thin air. The new experience is then solidified by insinuating that the reader has been familiar with it all the time. It is solidified and soon accepted as gospel truth, despite the fact that its conceptual components are incomparably more speculative than are the conceptual components of common sense. ... he built in this way a new world-view which was loosely (if at all!) connected with the preceding cosmology (everyday experience included)."²⁹

Feyerabend correctly asserts that Galileo constructed a new world-view by interlocking perceptual and conceptual components of experience into a totally novel whole. He achieved a new experimental gestalt, and realigned the meanings of traditional terms, so as to aid the development of the Copernican hypothesis. Unfortunately, Feyerabend's recognition of this interdependence of conception and perception leads him to conclude that comprehensive conceptual schemas cannot be compared:-

"Incommensurable theories, then, can be refuted by reference to their own respective kinds of experience; i.e. by discovering internal contradictions from which they are suffering (In the absence of commensurable alternatives these refutations are weak...). Their contents cannot be compared. Nor is it possible to make a judgement of verisimilitude except within the confines of a particular theory (remember that the problem of incommensurability arises only when we analyze the change of comprehensive cosmological points of view - restricted theories rarely lead to the needed conceptual revisions. None of the methods which Carnap, Hempel, Nagel, Popper or even Lakatos want to use for rationalizing scientific changes can be applied, and the one that can be applied, is greatly reduced in strength. What remains are aesthetic judgements, judgements of taste, metaphysical prejudices, religious desires, in short what remains are our subjective wishes..."³⁰

Feyerabend's objections to rational theory testing are revealing, because they show clearly the limitations inherent in any schema to test and compare theories that applies only deductive and inductive methods.

If conception and perception are mutually constitutive of one another then, any form of reasoning that appeals to perception to support conception, but not vice versa, must inevitably collapse or be sufficient only internal to a conceptual schema. The only grounds available for discarding a conceptual framework is conflict with observation; not that it compares unfavourably with an alternative framework. But such conflict cannot offer definitive refutation: we cannot be sure that the blame does not reside in those special conceptual commitments contained in the observational parts of the schema. There cannot even be any determination of the comparative inductive support of theories, or verisimilitude, except internal to a framework. This means changes in world views cannot be rationally mediated. We are confined to our intellectual and perceptual frames of reference and, when we change them, it is not because we have judged the comparative adequacy of different frames - it is a consequence of a change in our 'subjective wishes'.

It is not just a difference in perceived experience that pre-empts rational evaluation for Feyerabend; it is also the variance in the conceptual worlds of different theories. They are incommensurable because they deploy different languages. Even if the vocabulary they contain appears the same they use terms that have different meanings. Thus, as we saw earlier, Feyerabend assumes that theories like classical mechanics and the relativity theory are noncomparable. Views such as these have also been suggested by Kuhn, who has argued that paradigm changes cannot be mediated rationally. They involve different conceptual and perceptual worlds; and there cannot be communication across the gulf that separates paradigmatically defined communities.

The only way to explain paradigm changes is to look at the sociocultural influences that caused scientists to reject one paradigm and accept another; the internal rational evaluation of theories is insufficient to effect the change since such comparisons are not possible across the paradigm divide.

This despair of any possibility of rational theory testing leads Kuhn and Feyerabend to deny that there is any method that can mediate agreement between those who inhabit different conceptual worlds. All methods can be given only internally to a scheme of interpretation. Since there are no schema-transcending networks of meanings, perceptions or standards of evaluations, no scientific methodology is possible. This is why Feyerabend adopts an anarchist approach to method - which, in itself, is no method at all. Similarly Kuhn denies that there can be any rules to comparatively evaluate paradigms, because the paradigm itself is constitutive of the standards of science that are to be employed. All possibility of objective knowledge is denied by their relativistic conclusions. It is also a relativism that ultimately appeals to subjective wishes, or social cultural factors, to explain the acceptance and rejection of the most radically innovative theories of science.

This leads Feyerabend to reduce science to the metaphor of mythology (in the perjorative sense of unsupported and purely constructed knowledge, rather than conceptually and perceptually vindicated schemas of explanation); and Kuhn appeals to the metaphor of art (as a model of changing and contingent fashion, rather than the progressive expression of man's increasing awareness of his subjective world) to characterize the nature of scientific knowledge.³¹ Though both are rightfully critical of the cumulative model of scientific growth, they are also

advocates of the nonprogressive nature of science. What consensus there is, at any one time amongst the scientific community, is only achieved because of factors that are external to the scientific debate. Science does not offer us an example of incremental human awareness of the external world - it is merely a history of changing fashions determined by sociocultural forces or psychological wishes. Those who subscribe to different myths (or schools of art) in science can neither compare their views against a common experience, nor communicate with one another. This is the ultimate end in holistic empiricism of a dominant tradition in Oxfordian philosophy, a tradition that assumes every language to constitute a form of life that can be 'understood' only within its own frame; contact with alien forms of life is impossible.

We have seen that contact is possible - but not if we restrict ourselves to the vehicles of deductive and inductive reasoning. Nothing in these forms of discourse allows us to utilize alternative conceptions at the same time in order to test both of them. The reasoning involved in formal deductive logic explicitly excludes any possibility of simultaneously employing conflicting statements. Though we certainly cannot accept contradictory statements as parts of a single theory, conflicting frameworks can only be compared if we can reason and critically evaluate them at the same time. This is what dialectical reasoning allows us to do.

In constructing the background equivocating language we have to employ the competing schemas simultaneously. The language to effect communication between schemas is constructed out of them; so is the experience and the empirical basis employed to compare them. We are not required to leave the conceptual frameworks and appeal to a language, or

experience, that exists externally to either of them; neither are we forced to lock ourselves into one, or the other, of these frameworks in order to reason. We have to subject both frames of reference to conceptual analysis, and to exhibit the hidden presuppositions contained in them to a degree of depth not required by either deductive or inductive reasoning. This has to be done in order to create the background language of communication. Such a hermenentic - dialectical approach also facilitates a deeper understanding of both alternatives by actively pitting each framework against the other, and using each one to reveal the hidden presuppositions contained in the other.

Such a dialectical method facilitates the movement towards agreement and consensus out of initial disagreement and conflict. It is an active and creative movement that attempts to regress (or progress) towards a language in which agreement would be possible. This movement would also be a step towards a network of shared theoretical commitments because, given the expectational view of statements, a consensus on language is itself a consensus on theoretical presuppositions. It is out of, and through this, theoretical consensus that observational theories are derived, from which an empirical basis to compare the theoretical frameworks can be obtained. Thus the regression towards a background equivocating language is also a movement towards a consensus way of perceiving the world - it isolates the experience contingent on a particular framework from the one that is common to them both. It demarcates a background of language, theory and experience that is shared by the frameworks, from a foreground that is relative to each of them.

One of the basic differences between deductive logic and dialectical reasoning is that the former is only concerned with the form of statements; the latter takes into account both form and content. In deductive logic we confine ourselves to statements as isolated units from which other statements follow. The network of relationships that the statements, isolated for deductive purposes, have to the rest of the conceptual framework is irrelevant to the context of deduction. In dialectical reasoning - such as used in the construction of BEL - we have to take into account this total network of relationships. Thus both the content and form of statements become crucial.³² Indeed, given the expectational view of statements, we cannot separate form and content. The content of what a theory or statement offers is inextricably intertwined with its form: the informative and semantic functions are complementary aspects of a statement.

However, the formal nature of deductive reasoning makes it an important component of dialectical reasoning. This is because once we have constructed BEL by dialectical reasoning, deductive reasoning employed within BEL does not get us out of BEL. Deductive reasoning, being only formal and concerned with the structure of statements, does not change either the theoretical or observational meanings of the terms involved. Since these meanings in BEL are common to the frameworks out of which it has been constructed, logical deduction within BEL leads to statements that are also a part of it. Thus statements deduced from equivocating statements remain equivocating. This is extremely significant, for it shows that once a sufficiently large BEL has been created for two different conceptual frameworks this can be extended by deductive reasoning within itself. For example, by showing that

Saccherian postulates furnish the BEL for Euclidean and hyperbolic geometries, we can assume that all the theorems of Saccherian geometry are also theorems of these other geometries. The widening of BEL also widens the scope of the BEL-experience and, hence, the empirical basis against which different conceptual frameworks can be compared.

The possibility of dialectical regression to BEL for different frameworks exposes the crudity of the dichotomy commensurable versus incommensurable. Alternative world-conceptions are neither only commensurable nor only incommensurable. They have components that are commensurable and others which are not, and dialectical reasoning has to be employed to separate and distinguish the two. The BEL and the BEL-experience constitute the commensurable background of alternative frameworks. What cannot be included in BEL is the incommensurable foreground specific to each framework. The either/or alternative offered by holistic empiricists assumes that theories that are not commensurable must be incommensurable. This ignores the subtle, and often complex, ways in which different conceptual frameworks relate to one another.

Atomic empiricists suffer from the same naivety. They assume that frameworks are commensurable in every respect, because there is a language to effect this commensuration, and an experience to test these frameworks, that pre-exists the frameworks themselves. This is also the view of Popper and Laudan who presume that the theories that effect commensuration pre-exist the theories being compared - the so-called background theories that are independent of the theories being tested. We have seen that such views are untenable. The commensurability of frameworks is not achieved by appealing to a language that pre-exists

the frameworks themselves - it is an achievement obtained by subjecting the frameworks to dialectical analysis. Given alternative frameworks we reason towards commensuration by isolating a language in which this is possible. In moving towards commensuration we also isolate the features of the frameworks that are incommensurable. No amount of deductive or inductive reasoning within the frameworks can achieve this.

By using dialectical reasoning we recognize that, even if meaning and experience is always offered internal to frameworks, the commensurability and comparability of different frameworks is, nevertheless, possible. This is not done by an appeal to something that transcends either framework, but by constructing a commensurable network of meaning and experience out of the frameworks themselves. There is no foundational experience either; or a relatively foundational one independent of the theories being compared. There is no foundational meaning since all linguistic conventions are empirical claims. But this does not mean that there is no background of meaning, or experience, to test different conceptual representations. We create such a background out of the very frameworks that are involved; created out of them to be commensurate to them.

The use of the dialectical method and dialectical argumentation reveals the critical aspect of all scientific discourse. Where there is conflict between frameworks we do not presume the validity of either framework. Recognizing the interdependence of conception and perception we cannot, like the atomic empiricists, invoke a transcendent network of foundational conceptions (given as meaning postulates); or a transcendent set of foundational experiences. Alternatively, we do not need to pursue the relativist route of locking ourselves into a given conceptual

and perceptual frame of reference. We can utilize the frames in conflict to mediate the conflict, by creating out of them the language and experience required to achieve this. We presume that, where theories disagree - where the experience and meanings (theoretical claims) they offer lie outside the dialectically constructed background - they cannot appeal to their specific presuppositions, or observations made in the context of these, to effect the comparison. It is precisely the acceptability of these presuppositions and perceptions that is in question. Instead we have to move towards the background equivocating language, and the experience furnished through this language, to test the theories. A straightforward hypothetico-deductive approach is uncritical of perception; holistic empiricist thinking is not critical enough to mediate between frameworks. The first has to presume the validity of some absolute (or relatively absolute) framework; the second commits itself to some framework as if it were an absolute.

Given the mutual interplay between world-conceptions and world-perceptions, we cannot take the dialectical method to be merely an instrument for achieving co-understanding and commensurability between frames. It is an essential technique for the progress of science. For, if all experience is theory-impregnated and constituted by fundamental presuppositions (which are often tacit and hidden) then the only way to advance science is to create alternative conceptions that can be used to test those fundamental commitments. Thus the progress of science demands the proliferation of alternative viewpoints, each of which can be exploited to test the presuppositions of others. It is only through such a pluralist interaction of theories that scientific consciousness can grow and scientific objectivity can be achieved. Feyerabend is one

of the few philosophers of science who recognizes the important role of theoretical conflict for theoretical development, but his excessive holism prevents him from carrying such a program very far. Like the foundationalists he too presumes that, if perception is theory-laden, we are all locked within incommensurable conceptual systems. Although he encourages theoretical pluralism as a means of advancing science, his radically incommensurabilism prevents him from talking of advancement at all. Pluralism becomes for him merely a means of discovering new phenomena - like Brownian motion³³ - that are 'refuting' of previous theories. Even in this case it is difficult to understand how he could conceive such refutation to be possible since the experiences obtained through one theory, are reported in a language incommensurate to the theory they purport to 'refute'!

Furthermore, refutation - even the weak sort envisaged by Feyerabend - is not the way scientific theories get eliminated in science. As pointed out by a number of writers - Kuhn, Lakatos - theory elimination always occurs only when an alternative theory has been found to replace a predecessor.³⁴ But if alternative conceptual schemas cannot be compared it is difficult to see how this can happen. This is probably why Feyerabend still clings to weak refutation long after he has undercut all the supports of Popperian falsificationist epistemology.³⁵ Given that theories can be made commensurate, however, it becomes even more possible to recognize the crucial role of a pluralist methodology. It is only in this manner that we can test the assumptions involved in any world-conception and world-perception; it is also necessary to make progress in science because, without the presence of an alternative, no theory would ever get eliminated. The reason why

elimination and replacement of theories go together will become even more evident in the subsequent chapter, where I deal with the problem of rational theory admission, acceptance and elimination.

Thus the denouement of method that Kuhn and Feyerabend announce is misleading. For them method meant the employment of deductive or inductive logics; or the hypothetico-deductive method. Given the inadequacy of these methods to mediate across theories, and the recognition that all reasoning involved presuppositions, these philosophers assume that no rational method of theory comparison is possible. Dialectical reasoning offers such a method, a method which explicitly acknowledges that all reasoning between alternatives involves presuppositions. Basic scientific norms may change, but these changes themselves are mediated dialectically. This is a method that does not presuppose anything, except what is presupposed by the alternatives offered. It does not offer any norms independent of theories, but uses those offered by the theories themselves to compare them. It does not presuppose any experience independent of conceptual frameworks, but uses the experience offered by conflicting frameworks to test these same frameworks.

3.6 Conclusion

This chapter was concerned with developing a theory of meaning that is neither only atomic nor only holistic - a holarchic theory. The holarchic theory leads us to recognize that scientific terms cannot be separated into two classes on the basis of being observational and theoretical. Instead, all terms can possess theoretical and observational meaning. The observational meaning is given by empirical

associations to units of gestalt (in the context of observational theories); the theoretical meaning is defined implicitly by every sentence in which the term occurs. Thus all terms can possess an atomic and a holistic component of meaning.

The holarchic theory enables us to offer a dynamics of meaning change. It is the incompatibility between the theoretical and observational meaning of terms that often causes the greatest changes in terminological meaning. Such alterations are often involved in scientific revolutions where radical theory changes occur. Nevertheless, even during periods of normal science meanings are changing gradually as our conceptual frameworks evolve. In spite of such changes, it is possible to compare different frameworks by appealing to a language of shared observational and theoretical meaning. The background equivocating language can also be used to furnish the observational theories through which the data employed to compare them can be obtained. This language is constructed out of the frameworks in competition - and it is a language that also evolves as the theories used to construct it change and develop. In the following chapter we shall see how competing theories can be rationally compared against the experience that is furnished through their background equivocating language.

CHAPTER FOUR

SCIENTIFIC EXPLANATION AND SCIENTIFIC RATIONALITY

4.1 Introduction

In this chapter I want to propose an account of scientific explanation and scientific rationality that does not require us to assume two of the fundamental presuppositions of the atomic empiricist tradition. In the first place, the logical empiricists (who may be deemed to be the culmination of the atomic empiricist tradition) viewed the connection between an event and the theory that explained it as a fairly straightforward one, in which the statement describing the event was logically deduced from the theory (and statements that specified the initial conditions of the situation in which the event occurred). This view furnishes too simplistic an account of scientific explanation. A theory usually explains an event only in conjunction with other auxiliary theories. Hence an event is explained, not by a single theory, but by a network of theories.¹ Furthermore, the tradition assumes that the report of the event is an unproblematic given. However, observational theories are always involved in reporting an event.² This makes the logic of explanation somewhat more complex than had been envisaged by the logical empiricists.

Secondly, the traditional atomic empiricist view assumes that, when one theory superseded another, the successful domain of the new theory subsumed the domain of success of its predecessor. Thus, it assumes that the new theory is always one which, not only explains all the events explained by the earlier theory, but also a new class of events

not explained by it.³ However, a theory is often replaced by another whose content may not include all of the content of the preceding theory. Instead, it may be replaced, not by a single theory, but by a theory complex that divides its domain into subdomains that are shared by the theories of the new complex.⁴

The model of rationality I propose requires us to give up the view that theory replacement involves domain subsumption. If there is no domain subsumption we cannot compare the two theories by comparing their success in explaining events in terms of their individual absolute scope - whether this scope be measured in terms of generality, predictive power or explanatory power of the theories concerned. This is because they do not share a common domain of events. They only share that class of events that belong to the domain of both theories. Outside this shared domain of events the theories are not in competition.

Instead, the comparison of theories has to be effected by, what I shall call, the relative explanatory indispensability of one theory with respect to another. The notion of relative indispensability allows us to determine, in a direct way when scientific theories may be said to be admissible; when they have to be rejected; why one theory may be said to be preferred to another; and the conditions that have to be satisfied for a theory to be deemed worthy of acceptance. The measure of relative indispensability may also be used to measure the degree of acceptability of different scientific theories.

The account I shall give will also reveal why scientific revolutions cannot be interpreted as, either a cumulative increase of knowledge, or a disjunctive change where new theories have no features of continuity with their predecessors. Revolutions exhibit both cumulative

and disjunctive features; they can be precisely identified by an appeal to their background equivocating language. Thus neither the traditional belief in radical commensurability of pre- and post-revolutionary science; nor the recent views of radical incommensurability, adequately reflect the way experience and language change as a result of a scientific revolution.

4.2 The Dialectical Model of Scientific Explanation

The traditional logical empiricist model of explanation assumes that no successful theory of science can come into conflict with an observation statement, even though it allows that it would often be unable to explain all the events in its domain. Thus, whether it was held that verifiability, confirmability or falsifiability was the criterion to be used to establish the epistemological credentials of a scientific theory, it was generally conceded that scientific theories were, at least, directly falsifiable by observation. This, of course, is a conclusion that follows from the way scientific explanation was conceived within the framework of logical empiricism. To explain an observation was to deduce the result of the observation from a particular theory and specified initial conditions. Since the observation report and initial conditions could be given in a neutral incorrigible observation language, any disconfirmation of a theoretical deduction would refute the theory involved in the explanation.

The explanatory schema may be formulated as follows:-

Theory + Initial Conditions \rightarrow O

O' is observed

If O' is identical to O , then the theory may be said to explain O' . If O' is not- O , then since O' and the initial conditions are incorrigible - being pure observation reports - the theory being tested is refuted.

This is, of course, an extremely simplistic account of the logical empiricist view and I am not claiming that all logical empiricists held observation reports to be incorrigible. Neurath, for example, rejected the view that there were any statements in science - including observation reports - that were verifiable with certainty.⁵

Nevertheless, basic to the logical empiricist position is the belief that observation reports somehow constitute an unshakeable foundation. Even Hempel, who concedes that 'science offers various examples (when) a conflict between a highly confirmed theory and an occasional recalcitrant experimental statement may well be revoked by revoking the latter rather than sacrificing the former, admits that he can offer no other 'fundamental standard' than that of falsificationism.⁶

Hempel's dilemma is understandable. This is because falsificationism offers the weakest form of constraint on theory construction that is possible within the framework of any epistemology that requires a foundational empirical basis. As Lakatos points out "all justificationists, whether intellectualists or empiricists, agreed that a singular statement expressing a 'hard fact' may disprove a universal theory, but few of them thought that a finite conjunction of factual propositions might be sufficient to prove 'indirectly' a universal theory."⁷ Even Popper, who modified his views in later years, adopts a similar view. He quotes with approval the mathematical physicist Weyl who wished 'to record (his) unbounded admiration for the work of the experimenter in his struggle to wrest interpretable facts

from unyielding Nature who knows so well to meet our theories with a decisive No - or with an inaudible Yes." Popper adds, "I fully agree."⁸ Braithwaite too adopts a similar view.

To what extent, then, should an established scientific deductive system be regarded as a free creation of the human mind, and to what extent should it be regarded as giving an objective account of the facts of nature? ...The form of a statement of a scientific hypothesis and its use to express a general proposition, is a human device; what is due to nature are the observable facts which refute or fail to refute the scientific hypothesis ...we hand over to nature the task of deciding whether any of the contingent lowest-level conclusions are false. This objective test of falsity it is which makes the deductive system, in whose construction we have very great freedom, a deductive system of scientific hypotheses: Man proposes a system of hypothesis; nature disposes of its truth or falsity. Man invents a scientific system and then discovers whether or not it accords with observed fact.

The view that theories can be directly refuted by single observation reports has been criticized by various philosophers. One line of argument against naive falsificationism traces its descent from Duhem, and has been more extensively developed by Quine. According to Duhem "the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions what he learns is that at least one of the hypotheses constituting the group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed."¹⁰ Similarly, Quine holds that given sufficient imagination we can save any theory from observational refutation provided we are willing to make an adequate alteration in our background knowledge. "Any statement can be held true, come what may, if we make drastic enough adjustments elsewhere in the system Conversely by the same token, no statement is immune to revision."¹¹

This famous Duhem-Quine thesis has been extensively discussed. It furnishes one of the fundamental arguments against any naive falsificationist view of theories. The argument schema for explanation that follows may be formulated thus:-

Main Theory + Auxiliary Theories + Initial Conditions \longrightarrow O

O' is observed

Here we have separated as the main theory the theory that is currently being tested. Nevertheless, it should be clear that if O' corresponds to O, so that we have a successful explanation, this success cannot be attributed to the main theory alone. The explanation is provided by a complex of theories that includes the main theory as only one of them. Thus the confirmation provided when O' corresponds to O is a confirmation of the theory complex as a whole.

Suppose it turns out that O' is not O. Then the explanatory theories, constituted by the main and auxiliary theories, make a prediction that is disconfirmed by experience. According to the Duhem-Quine thesis this disconfirmation cannot be attributed to any one of the theories on its own; it is the explanatory theory complex that is refuted. Since all explanations in science involve a complex of theories, no theory can be refuted by a single observation report. The modus tollens is directed against the explanatory system, but it does not indicate which of the explanatory theories has been refuted by the observation.

Another line of argument against direct falsificationism has been developed by Kuhn and Feyerabend. To do this they develop Hanson and

Toulmin's thesis¹² that all observations are theory-laden. They argue that since the observation statement used to test a theory is itself theory-laden, the failure of a theoretical prediction may be blamed upon the observational theories used to make the observation report rather than the explanatory theory. I.e., a conflict between a prediction and an observation leaves us uncertain as to whether it is the observational or explanatory theory that has been refuted. Since this is the case whenever a theoretical prediction disagrees with an observation report, direct falsification of scientific theories is impossible.

Attacking Popper's view that theories can be falsified Kuhn asserts that for this to be possible we must require that

...the epistemological investigator and the research scientist (must) be able to relate sentences derived from a theory not to other sentences but to actual observation and experiments. This is the context in which Sir Karl's term 'falsification' must function, and Sir Karl is entirely silent about how it can do so. What is falsification if it is not conclusive disproof? Under what circumstances does the logic of knowledge require a scientist to abandon a previously accepted theory when confronted, not with statements about experiments, but with experiments themselves?¹³

For Kuhn, however, the statements derived from observation are paradigm-dependent.¹⁴ Thus conclusive disproof of a theory is impossible because we may reject the theory used to make the observation. "All experiments can be challenged either as to their relevance or their accuracy... It is important that this should be so, for it is often by challenging observations and adjusting theories that scientific knowledge grows."¹⁵

The same argument against falsification is presented more explicitly by Feyerabend. He criticizes the supposition that there is an asymmetry between observational and theoretical statements in that the former are incorrigible and the latter are refutable. "Even if we do

test our theories against experience, we also often evaluate our experience in the context of new theories. He writes

both theories and observation statements are open to criticism... True we often test our theories by experience; but we equally often invert the process, we analyze experience with the help of more recent views and we change it in accordance with these views.¹⁶

This is possible because experience, just like our theories, contains natural interpretations which are abstract or metaphysical ideas. Thus when a theory appears disconfirmed by observation the blame may not belong to the theory, but in the theories used in obtaining the observation reports.

...a theory may be inconsistent with the evidence, not because it is incorrect but because the evidence is contaminated. The theory is threatened because the evidence contains unanalyzed sensations which only partly correspond to external processes, or because it is presented in terms of antiquated views, or because it is evaluated with the help of backward auxiliary theories.¹⁷

The Kuhn-Feyerabend thesis against falsification may be schematized as follows:-

Main Theory + Initial Conditions $\rightarrow O$

Observational Theory $\dashrightarrow O'$

where the broken arrow (\dashrightarrow) indicates that the observation report is not deduced from, but made in the interpretive context of, the observational theory. If O' is not O we are left uncertain as to whether the blame is to be attributed to the main (explanatory) theory or the observational theory. Similarly if O' and O correspond, then the credit for the successful prediction belongs to both the explanatory and the observational theory.

Both the Duhem-Quine thesis and the Kuhn-Feyerabend thesis are arguments against the doctrine that scientific theories can be directly falsified. Nevertheless, they are also suggestive of what an adequate model of scientific explanation should be. For they reveal that in any explanation it is not a single theory, but a network of theories that is involved. In the first place there are the theories used to deduce the prediction - the explanatory theories. Secondly, there are also those theories that are involved in making the observation - the observational theories. Any successful explanation requires that the statement deduced from the explanatory theories agree with the statement given to experience in the context of the observational theories.

Thus a complete schema for scientific explanation may be formulated as follows:-

Main Theory + Auxiliary Theories + Initial Conditions \rightarrow O
(Explanatory Theories)

Observational Theories \rightarrow O'

If O and O' correspond the explanation is successful; otherwise it is not. And a clash between O and O' leaves open the possibility that we may either have to modify the explanatory theories or the observational theories. It is the dialectical relationship between the explanatory and observational theories that determines the success of an explanation. In the traditional model this was not the case: a successful explanation was a direct confrontation of an explanatory theory with theory-free observational data. This has been recognized clearly by Lakatos:

...the clash is not between theories and facts, but between two high level theories: between an interpretive theory to provide the facts and an explanatory theory to explain them.... The problem should not be put in terms of whether a refutation is real or not. The problem is how to repair an inconsistency between an explanatory theory under test and the - explicit or hidden - interpretive theories. ...It is not that we propose a theory and Nature may shout NO; rather we propose a maze of theories, and Nature may shout INCONSISTENT.

Let us now examine some of the consequences that follow from adopting the dialectical model of explanation that has been suggested. In the first place, the dialectical model reveals clearly that there are no autonomous facts that are given directly to experience. In this sense it is in accord with the claim, that we have defended earlier, that all observations are theory-laden. Thus it denies that there is a foundational empirical basis against which theories can be tested. Nevertheless, this does not mean that it denies that theories have to be empirically tested. Clearly the success of the explanatory theories depends on their ability to enable us to deduce the results of observations, but these observations, however, are corrigible because they are made in the context of other (observational) theories.

Secondly, the dialectical model reveals clearly why the falsification of a theory by an observation report is not possible. This is because a disconfirmation between a statement deduced from the explanatory theories, by an observation report, does not indicate whether the modus tollens is to be directed against the main, auxiliary or observational theories involved in the explanation.

Popper has argued that falsification of an explanatory theory is possible even if all observations are theory-laden. To accomplish this Popper requires that scientists must accept certain basic statements by

a conventionalist decision:

Every test of a theory, whether resulting in its corroboration, or falsification, must stop at some basic statement or other which we decide to accept. If we do not accept some basic statement or other, then the test will have lead nowhere (Popper's emphasis).

He concedes that the decision is conventional, but argues that his conventionalism differs from that of Poincare and Duhem in an important respect. For the latter it is the acceptance of universal propositions that is determined by convention, while for Popper it is singular propositions that are accepted by convention.

It seems to me that Popper's argument for the falsifiability of theories involves a circularity. For the statements he accepts as basic by convention are not arbitrary; they are those that are delivered by interpreting experience in the context of certain observational theories. To conventionally accept them as incorrigible is to tacitly endorse the claim that these observational theories are certain, and therefore, verified. Clearly, if we had some doubt about these observational theories, we would have to have a similar degree of doubt about the observation statements they have been used to formulate. Thus when an explanatory theory is said to be definitely falsified by an observation report, we have to assume that the observational theory used to make the report has been definitely verified. However, one of the reasons that Popper adopted a falsificationist methodology was because he could not accept a verificationist one. Thus he seems to have imported through the back door what he so ceremoniously ejected out of his front door.

Furthermore, what is to prevent someone from treating the singular statements deduced from the explanatory theories as basic by convention,

and using them to test the adequacy of the observational theories? In this case it would not be an observation report that falsifies a theory, but a theoretical prediction that is used to reject an observation report and its associated observational theory. There have been a number of cases in the history of science where this has occurred. For example the Copernican theory vindicated itself only after it had lead to a vast modification in theories of mechanics, dynamics and optics.²⁰ Similarly the quantum and relativistic revolutions in this century required us to modify even the most basic categories of observation such as space and time. But if one can adopt, by convention, the singular statements delivered either by the observational or explanatory theories as basic, how can such a conventionalist strategy be used to test theories?

Furthermore, the dialectical model makes explicit the expectational nature of all observation statements. This is because its structural form reveals that an observation report is formulated by looking at an event through observational theories. These theories suggest how the event is to be experienced and labelled. I.e., they embody a network of demands on how the event should be represented. At the same time the event itself acts as a constraint on how it can be represented. Therefore, the observation report is a result of both demands on how the event should be represented and constraints on how it can be represented. It is an expectation about the representation of an event.

Finally, the dialectical model suggests that the growth of science does not involve a cumulative process where the domain of given facts is constantly enlarged. It involves a bootstrapping process where observational theories are used to test explanatory theories that may

later be used as observational theories to test other explanatory theories. Furthermore, new explanatory theories may also be used to test accepted observational theories. This means that every part of our knowledge is always open to revision. Furthermore, the very theories used to make observations to test other theories are themselves open to testing by these newer explanatory theories. This bears a close analogy to the development of technology. One uses a certain level of technology to make tools that may themselves be used to improve the tools that made them.

The history of science is replete with examples where this has occurred. The theories of Aristotelian optics were used to make astronomical observations that lead to the problems that Copernicus attempted to resolve with the heliocentric theory. The theory itself required an extensive modification in mechanics, dynamics and optics. Similarly, the theories of classical physics were used to design experiments and make observations that lead to the relativity and quantum theories. Nevertheless, these theories themselves required us to modify the most fundamental assumptions of classical physics. In the same way modern elementary particle physics presupposes a number of theories in formulating any observation report about microevents. Nevertheless, there is no reason to suppose that these reports may not be used to suggest new theories that may later require us to reject the quantum theory, or present-day relativistic electrodynamical theories.

Let us now examine, in detail, an example from the history of science that lends support to the above interpretation of scientific explanation. Consider the Bohr model of the atom first proposed in 1913. This was inspired by the brilliant work of one of the greatest

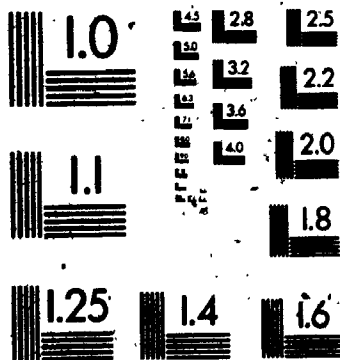
experimental physicists of this century - Ernest Rutherford. By bombarding a thin gold foil with fast alpha particles Rutherford showed that the large deflections suffered by these particles could only be explained by supposing that they had interacted at close range with a positively charged mass. He proposed that his results could be accounted for if all atoms were considered to be made up of a central positive nucleus surrounded by orbiting electrons whose mass was much smaller than that of the nucleus. However, this model was in conflict with both classical mechanics and classical electromagnetic theory. In the first place it was not possible for a system of point positive charges to be in stable equilibrium. Secondly electrons in orbit are accelerating charges and should, according to Maxwell's theory, emit electromagnetic radiation. In doing so they would gradually lose energy and ultimately spiral into the nucleus. The fact that the nucleus had to be positively charged in order to account for Rutherford's experimental results indicated that this did not happen. Bohr's theory was designed to explain the stability of the Rutherford atom, but in the process it initiated a further development away from classical physics.

Bohr first developed his theory for the hydrogen atom. Taking the cue from Rutherford's model he proposed that the hydrogen atom was constituted by a central positively charged nucleus around which a single electron was in orbit. He further assumed that there were certain orbits - what he called 'stationary' states - in which the electron could exist without radiating any energy. He also assumed that the frequency of the revolution of the electron in any one of them was an integral multiple of $1/2h$ where 'h' is Planck's constant. Thus it was possible to calculate the energy of the electron in these various

orbits. Bohr assumed that when an electron made a transition from an orbit in which it has higher energy to a lower energy orbit, the energy loss was emitted as a photon. Then using Planck's relationship between the energy of a photon and its frequency, it was possible to determine the frequency (and, therefore, wavelength) of the emitted photon. It was an extremely elegant extension of the planetary model to atoms. The only difference was that the negatively charged electrons that were held in place by the positive nucleus could only exist in certain well-defined orbits. When they 'jumped' from a larger orbit where they had higher energy to a smaller one, they radiated the lost energy as a photon, and this emitted radiation could be detected and its wavelength measured.

Spectroscopic studies in the 19th century had already given abundant information on such radiated energies. When an electric current was discharged through a tube containing hydrogen, it was observed that the gas emitted light whose frequencies could be measured by passing it through a prism that provided a dispersed spectrum. Using the Bohr model we can visualize the current as a stream of free electrons that knocked the electrons, bound to the hydrogen nuclei, into higher energy orbits. The light given off by the hydrogen gas was due to these bound electrons returning to lower energy levels. The frequency of the light emitted by hydrogen should, if the Bohr model were correct, correspond to the frequencies predicted by the theory. One of the most impressive successes of the theory was its explanation of the Balmer series that corresponded to the visible part of the spectrum of hydrogen. Moreover, it also predicted the Paschen series in the infrared, observed in 1908, and the Lyman series in the ultraviolet that was observed in 1914.

3



Let us examine Bohr's explanation of the results of spectroscopic investigation in closer detail. The main theory under test here is, of course, Bohr's theory of the atom. However, to explain the spectrum of hydrogen, Bohr had to assume both Newton's theory of dynamics and Maxwell's electromagnetic theory in order to calculate the energy of the electron in stable orbit. Thus these theories were employed as auxiliary theories even though their range was deliberately circumscribed. It is this combination of theories that is used to calculate the frequency of the radiation emitted by electron transitions in the hydrogen atom.

Furthermore, the Bohr predictions are tested by assuming another complex of theories that enable us to compute the frequencies of the radiation emitted by hydrogen when excited in a discharge tube. To know that the gas in the tube is hydrogen we have to appeal to theories of chemistry; to determine that the radiation emitted has a frequency is already to suppose a wave-theory of light; and theories of optics are involved in computing this frequency and designing the equipment to detect it. Thus the observation reports that give the frequencies are themselves highly theory-laden. To say that a line of a particular frequency has been observed in the hydrogen atom is to make a highly theoretical statement which would have no meaning to someone who did not understand these theories.

Soon after it was proposed it was noticed that the Bohr model was unable to explain a series of lines detected by Pickering in 1896 in the star ϵ -Puppis. Pickering had identified these lines as belonging to hydrogen because it "is so closely allied to the hydrogen series that it is probably due to that substance under conditions of temperature or

pressure as yet unknown."²¹ This was supported by Kayser who argued that it would mean that hydrogen had two series which "then corresponds remarkably with the ~~other~~ elements".²² He explained that the series had not been observed in experiments on the earth because the temperatures in the Geissler tubes was not sufficiently large. As pointed out by Jammer, such a series was more problematic than the fact that all the lines predicted by the Bohr model had not been observed. In the latter case the absence of experimental results could be attributed to experimental inadequacies; one could not do this with lines that had actually been observed.²³

Bohr resolved the problem by showing the untenability of the observational theories used in making the observation. He suggested that the series was not due to hydrogen, but to contamination by helium. He predicted that the same lines should be observed in a tube filled with a mixture of helium and chlorine, and that they were due to ionized helium atoms. This was subsequently confirmed by different experimenters - Evans, Fowler and Paschen. In this case we have an apparent disconfirmation of a network of explanatory theories by observation resolved by altering the observational theories.

On the other hand a conflict between observation and prediction may be resolved by altering an auxiliary theory. It was pointed out by Moseley that the Bohr model did not give an account of the fact that the spectrum of hydrogen was not made up of single lines, but of a series of doublets. This had been noticed by Michelson as early as 1891. He showed that the lines in the hydrogen spectrum occurred in pairs in which one line of the pair was considerably weaker in intensity than the other. This problem was solved by Sommerville. He did this by arguing

that, since the electrons in the hydrogen atom are moving at extremely large velocities, we have to use relativistic rather than Newtonian dynamics to compute their energy levels. Such an interpretation meant that the masses of the electrons would alter as they changed orbits, and also that the perihelion of their orbits would not lie in a single plane, but would advance with time. This relativistic calculation showed that the energy levels of the hydrogen electron were multiplets, and explained the fine structure of the lines noticed by Michelson. Thus, a contradiction between the value computed from a network of explanatory theories and the observed result was eliminated by changing from Newtonian to relativistic physics, i.e., by replacing an auxiliary theory by a different and more adequate theory.

Finally, however, it was the Bohr theory itself that had to be discarded in the face of observation. This was because it could not explain the doublet structure of the alkali atoms, where it was observed that there were twice as many quantum states as were predicted by the Bohr theory. Nor could it explain the anomalous Zeeman effect which showed that a single line of an atomic spectrum splits into three lines in the presence of a magnetic field. The central line remains undisplaced in the position of the original line, while the two new lines are displaced from this line at a distance proportional to the applied field. This was explained by Uhlenbeck and Goudsmit in 1926 with their famous postulation of a new quantum theoretical property of the electron - its intrinsic angular momentum or spin. The same electron in the same orbit would be in a different quantum state depending on whether this spin was up or down. This accounted for the doublets observed in alkali spectra, and for the line triplets observed in atomic spectra in the

presence of an external magnetic field.²⁴

The above example illustrates that, whenever a theory is said to provide an explanation of an event, what is actually involved is not only the theory but also other auxiliary and observational theories. In the event of a conflict between the deduction from an explanatory system of theories and an observation report we can never be certain which of the theories - the main, auxiliary or observational - have to be modified to resolve the inconsistency. The only way the problem can be dealt with is post hoc - after someone has produced a change in one of these theories that leads to a successful explanation. It is difficult to see, how context-independent general rules can be provided to determine which of the theories require modification when there is a conflict between observation and our theories.

4.3 Theories and Their Domains

A theory can only be required to explain an event if the event is considered to belong to the domain of the theory. The question of what belongs to the domain of the theory is not one that is easy to solve. Theories do not often automatically specify their domains at their inception. Only after a problem has been solved by a theory can we be categorically certain that it belongs to its domain. This is because a theory usually solves a problem only in conjunction with other auxiliary theories. Therefore, an event may not be seen to belong to the domain of a theory until the emergence of an appropriate auxiliary theory.

Thus in the course of its history the notion of what constitutes the domain of the theory may be radically changed. Statistical thermodynamics was considered in the 19th century not to apply to

radiant energy; Planck and Einstein extended the theory to include radiation phenomena. Planck did this by adding the hypothesis that radiant energy was emitted and absorbed in discrete packets, and according to Einstein, it was also made up of discrete packets. When Maxwell developed his electromagnetic theory he did not intend to include optics in his domain. This seminal discovery, that greatly increased the domain of his theory, was made possible only by adding the auxiliary hypothesis that light was a form of electromagnetic radiation. Such a hypothesis was suggested by the equality of light velocity to the velocity of electromagnetic waves, but it was not until Hertz's famous experiments of 1887 that it obtained experimental support.²⁵

Similarly the domains of theories may be restricted by new discoveries. Nineteenth century chemistry rejected the view that material interactions could ever lead to the transmutation of elements. This fundamental doctrine was shattered by Rutherford in the early years of this century. This hypothesis is now confined only to chemical reactions where the energies of the reactions are small. Similarly the doctrine that there cannot be spontaneous generation of life was espoused by 19th century biologists after Pasteur; it is now confined to the view that complex forms of life cannot be so generated. However, simple self-replicating molecules that are the primordial ancestors of all life could arise spontaneously. In each of these cases the domain of a hypothesis was restricted by expanding knowledge, but the hypothesis itself was retained within the restricted scope.

A change in the domain of events requiring explanation by a theory may also arise from a change in observational, rather than auxiliary, theories. Comets were seen in the Middle Ages as sublunary phenomena

that meteorology was required to explain.²⁶ The development of the telescope and the new theories that were used to interpret telescopic observations required us to view them as astronomical phenomena. One of the most impressive successes of Newtonian physics in the 18th century was the prediction of the return of Halley's comet. Thus as Shapere indicates

...considering a body of information to be a domain is itself a hypothesis that may turn out to have been mistaken. In other words, that a body of information constitutes a domain is itself a hypothesis, and may ultimately be rejected.²⁷

Similarly Whittaker shows how difficult it had been in the early years of the 19th century to decide if the different forms of phenomena labelled electrical - those connected with physiological, magnetic, calorific, chemical and mechanical events - really belonged to the domain of a single theory. It was only Faraday's experimental and theoretical genius that finally resolved the issue in favour of a unified theory.²⁸

In this discussion I do not want to go any deeper into the matter of identifying scientific domains. However, it should be clear by now that some of the issues connected with this problem can be understood if we take into account the explanatory model that was proposed in the previous section. For that model supposed that whether a theory can explain an event depends on the auxiliary theories available. Furthermore, even if an event occurs independently of our theories, the categorization of that event depends crucially on the sort of observational theories that are employed. Thus, given this model of explanation, we cannot be surprised if the domain of a theory may alter dramatically without any change in the theory itself, provided there

have been changes in the auxiliary theories or the observational theories used to report events.

Let us now consider the standard view of what happens when a theory is replaced by a more successful one. This view assumes that the domain of success of the new theory includes the domain of success of its predecessor. I.e., the new theory not only explains all the events dealt with by its predecessor, but also explains other events left unexplained by the earlier theory. Such a claim is made by Collingwood:

If thought in its first phase, after solving the initial problems of that phase, is then through solving these brought up against others which defeat it; and if the second solves these further problems without losing its hold on the solution of the first, so that there is gain without any corresponding loss, then there is progress. And there can be progress on no other terms.²⁹

Such a view is also espoused by Popper and Lakatos. Popper argues that when one theory replaces another, the new theory must entail every fact entailed by its predecessor.³⁰ Similarly, Lakatos' methodology explicitly requires that successor theories include, in their domain of success, the events explained successfully by their predecessors. He requires that theories be both theoretically and empirically progressive when compared to the ones they replace. To be theoretically progressive the new theory must have excess empirical content over its predecessor. To be empirically progressive some of this excess content must have been corroborated. For Lakatos a progressive problemshift "is both theoretically and empirically progressive, and otherwise it is degenerating".³¹

The claim that successor theories subsume under their domain of success that of their predecessor has been criticized by a number of philosophers - notably Kuhn and Feyerabend. According to Kuhn "new

paradigms seldom or never possess all the capabilities of their predecessors (though) they usually preserve a great deal of the most concrete parts of past achievement and they always permit additional concrete solutions besides."³² Whereas Kuhn's criticism is based upon historiographic data revealing that actual episodes in science do not conform to this requirement, Feyerabend's critique embodies methodological elements as well. "Theories which effect the overthrow of a comprehensive and well-entrenched point of view, and take over after its demise, are initially restricted to a fairly narrow domain of facts, to a series of paradigmatic phenomena which lend them support, and they are only slowly extended to other areas. ...it is also plausible on general grounds: trying to develop a new theory, we must first take a step back from the evidence and reconsider the problem of observation."³³

Stepping back for Feyerabend involves questioning the observational theories that could be used to claim that the evidence is against the new theory. Thus the new theory may appear to have a smaller content only because we have not developed an appropriate set of observational theories.³⁴

The history of science furnishes us with examples of cases where the content of a successor theory did not include all the content of its predecessor. Take the case of the Lorentz electron theory. This theory was concerned with the structure of matter, the electrodynamics of moving bodies and atomic spectra. It was a highly successful theory, which, as we saw earlier, was subsequently replaced by a theory of elementary particles which dealt with the constitution of matter; the quantum theory which explained atomic spectra; and Einstein's theory of relativity which superseded Lorentz's in problems connected with the

electro-dynamics of moving bodies. Thus, the Lorentz theory was not replaced by a single theory. Instead a number of theories shared the Lorentzian domain between them.

Alternatively, a theory may have some part of its domain replaced by a new theory. Einstein's photon theory explained phenomena like the photoelectric effect, black-body radiation and others, but the domain of events it was concerned with was only a subdomain of the wave theory. In particular these were the phenomena that were anomalous to the wave theory. Einstein's theory did not include in its domain of success those phenomena that were so elegantly accounted for by the wave theory - interference, diffraction and polarization effects.

Of course there are cases where one theory supplants another by subsuming within its domain the domain of the earlier theory. However, the standard view is mistaken in supposing that this is generally the case. In general, we can only say that there is an overlap in the domain of events contained in a predecessor and successor theory. For this reason, as we saw in a previous chapter, we cannot evaluate the two theories by merely comparing their overall performance with respect to one another in terms of values like simplicity, explanatory or predictive power, or generality. Such a comparison would be meaningful only if the theories compared deal with the same total domain of phenomena. Furthermore, even if we could decide that one theory performs better than the other in some overall sense, this cannot be grounds for rejecting the other. The other theory may still be able to explain events outside their shared domain that the competitor does not even deal with.

4.4 The Rational Evaluation of Theories

I shall now develop a model of scientific rationality that does not require us to assume domain subsumption; the cruciality of norms like generality, predictive power or explanatory power of theories; and which recognizes that theories do not get admitted or accepted solely on the basis of how they compare with the evidence, but also in terms of how they compare to other theories. We have seen that Einstein's photon theory was considered admissible precisely because it could account for events that could not be explained by any of the theories of classical physics. Neither classical thermodynamics nor Maxwell's electromagnetic theory, or a combination of the two of them, could handle the problem of blackbody radiation or the photoelectric effect. Thus, relative to the complex of classical theories, Einstein's photon theory was indispensable. For this reason it was considered an admissible theory. But how would one compare, say, Einstein's theory against thermodynamics? How would one measure their relative success? The two theories certainly have an overlapping domain - they both seek to explain blackbody radiation - but they do not have the same domain. Thermodynamics deals with problems of gas behavior and chemical reactions which the photon theory does not. To compare them in terms of their success over the total domain would be absurd - they are not competitors outside their shared domain. If we give up the notion of comparing them by measuring the total scope of each theory, how can they be compared?

Consider thermodynamics - its explanatory success depends on using it in conjunction with Newtonian mechanics and classical electromagnetism as auxiliaries. Let us call this set of theories (thermodynamics

inclusive) the thermodynamic theory complex. The thermodynamic theory complex includes Newton's theory of mechanics, Maxwell's electromagnetic theory and, most important, thermodynamics. Let the scope of thermodynamics be the number of events explained by thermodynamic theory (in conjunction with the other theories of its complex); and the scope of the thermodynamic complex be the number of events explained by all the theories of the complex taken together. Thus the scope of the complex includes the scope of thermodynamics, but not vice versa. Similarly we can define the scope for Einstein's photon theory and the photon theory complex - where the complex would include those parts of classical physics that are used in conjunction with the photon theory to achieve its explanatory success.

We have seen that Einstein's photon theory was admitted because it was able to explain events left unexplained by the theories of classical physics, - including classical thermodynamics. Let us say that the more events of this sort that the photon theory was seen to explain the more indispensable it would appear relative to thermodynamics. Let us fix a measure for the amount of indispensability of Einstein's theory relative to thermodynamics by making it the scope of the photon theory outside the thermodynamic theory complex. (Note that it is the scope outside the thermodynamic complex, and not merely the scope outside the scope of thermodynamics, that defines the measure). This is the number of events explained by the photon theory that are not also explained by the thermodynamic complex. Analogously, we can define the indispensability of thermodynamic theory relative to the photon theory as the scope of the former that lies outside the scope of the photon theory complex. Then the two theories can be compared in terms of what I shall call

their relative explanatory indispensability (REI), defined as follows:-

$$REI = \frac{\text{Scope of } T_1 \text{ outside } T_2\text{-complex}}{\text{Scope of } T_2 \text{ outside } T_1\text{-complex}}$$

where, T_1 is classical thermodynamics and T_2 is Einstein's photon theory. (The relation, however, would be applicable to comparing any two theories of science).

At this point the reader may raise an objection. It may be said that if different theories give rise to different experiences of the world how can it be possible to determine that part of a theory's scope that lies outside an alternative theory complex? This is where the background equivocating language becomes crucial. We have to construct the BEL for both the T_1 -complex and the T_2 -complex, and it is only the data given in BEL that are relevant to the comparisons of the theories. Data that are not given in BEL, but in terms of one or the other theories, are open to question as to their veridicality because their legitimacy is itself in question. We cannot presume that those experiences that are possible in the T_1 -complex, but not recognized by the T_2 -complex, can be used as evidence against the latter: this is to presume the preferability of the former to the latter even before a comparison has been effected. Thus, in the subsequent discussion, it will always be assumed that the data used to compare theories is only that which is furnished through BEL - theories outside BEL are not employable as observational theories since their very acceptability in offering data is itself under test.

The notion of relative explanatory indispensability enables us to characterize directly the sort of rational decisions involved in comparing scientific theories. It enables us to answer questions like: why are some theories deemed admissible while others are not? How do theories get eliminated? Given two admissible theories how do we compare their relative merits or preferability? When do theories that are admitted become successful enough to be rationally acceptable? These are questions that pertain to the contexts of pursuit, comparison and acceptance of theories. I shall begin by proposing a set of definitions, and then subsequently show why these offer an account of scientific rationality that captures many of the features recognized by recent historiographic studies - features that do not conform to the traditional views of scientific rationality.

Let us say that:-

1. A theory T is admissible if its relative explanatory indispensability is nonzero with respect to all other admitted theories. ($REI = 0$)
2. A theory T_1 is preferable to another theory T_2 if its relative explanatory indispensability with respect to T_2 is greater than unity ($REI > 1$)
Also T_1 is superlatively preferable to T_2 if its relative explanatory indispensability with respect to T_2 is very much greater than unity ($REI \gg 1$)
3. A theory is acceptable if it is superlatively preferable to every other available theory.

[The notion of preferability I have defined is not transitive. It is a technical notion defined as above.]

(a) Rational Admittance

We have seen that a theory is often considered for admission, regardless of its scope of success, because it can explain some events that cannot be accounted for by other theories we have at our disposal. The notion that it is admissible if its relative explanatory indispensability is nonzero with respect to all other theories certainly captures this observation. It does not matter what scope the other theories have; so long as the theory has some scope not included in that of other theories it would have to be deemed acceptable. This is why Einstein's photon theory, Planck's theory of radiation and the Bohr model of the hydrogen atom were initially admitted: in spite of the fact that their scope, at their inception, was much smaller than the classical theories that were their intended competitors.

The common view is that a new theory can be admitted only if it subsumes the domain of its earlier competitor - i.e., the scope of the older theory outside the new theory complex should be zero. Then the relative indispensability of the new theory with respect to its predecessor would be infinite; but this is only a special case of admissibility. In general theories do not subsume the domain of earlier theories. Furthermore, even where the scope of the new theory is intended to be developed so as to subsume the domain of the earlier one, it is unreasonable to require that it must at its inception solve all the problems dealt with by its earlier rival.³⁵ The Tatter has been developed over a much longer period of time. Its success in many areas was achieved laboriously. The new theory may not have had sufficient time for adequate development so as to show that it can successfully deal with all the problems dealt with by its predecessor.

Time may also be required to develop adequate auxiliary and observational theories so that the full potential of the new theory may be explicitly recognized. Some of the difficulties the theory confronts may be due, not to inadequacies of the theory itself, but to the use of inappropriate observational theories, or the absence of adequate auxiliary theories. Given time the new theory may be more successful than its rivals, for it may guide us in developing better auxiliary and observational theories.³⁶

Another reason why new theories cannot be required to subsume the domain of an earlier one is that it would not allow for theories whose domains are not intended to include all the domain of their rival. We have already seen that quantum mechanics and the theory of relativity were both rivals to the Lorentz electron theory, yet neither of their domains subsumed the Lorentzian domain. Instead different parts of the Lorentzian domain were explained by these two different theories, and other parts of its domain were relegated to modern elementary particle physics. Similarly the phenomenon of radiation, convection and conduction were explained by the caloric theory, but its rival - the kinetic theory of heat - was, in the nineteenth century, not concerned with radiation phenomena. These were, instead, considered to belong to the domain of the ether theories. If new theories can divide up the world of events into different domains, it would entrench conservatism in science to require that the more recent theories subsume the domain of success of some earlier theory.

The criterion of relative explanatory indispensability allows us to recognize that there can be rational criteria for admitting a theory even if a new theory is allowed to have less content than an earlier

rival. This would not be possible if we were to employ criteria like total predictive or explanatory power, or generality to compare theories. Feyerabend has argued that, since nearly all novel theories must necessarily adopt content-decreasing strategies, they must also adopt anti-rational techniques to win support against their rivals.

...a new period in the history of science commences with a backward movement that returns us to an earlier stage where theories were more vague and had smaller empirical content. This backward movement is not just an accident; it has a definite function; it is essential if we want to overtake the status quo, for it gives us the time and the freedom that are needed for developing the main view in detail, and for finding the necessary auxiliary sciences.

This backward movement is indeed essential but how can we persuade people to follow our lead? ... It is clear that the allegiance to the new ideas will have to be brought about by means other than arguments. It will have to be brought about by irrational means such as propaganda, emotion, ad hoc hypotheses, and appeal to prejudices of all kinds. We need these 'irrational means' in order to uphold what is nothing but a blind faith until we have found the auxiliary sciences, the facts, the arguments that turn faith into sound 'knowledge'.³⁷

However, there is no need to adopt Feyerabend's anti-rationalist claims even if we acknowledge his insight that new theories often involve a decrease in content with respect to their predecessors. Furthermore, there are even stronger ways in which a theory can prove itself than merely explaining events unexplained by any predecessor - it can resolve some of the problems that appeared to disconfirm an earlier rival. We have seen that all theories always confront events that are logically incompatible with their predictions. These are not grounds for considering a theory refuted because we cannot decide, unambiguously, whether the blame is to be attributed to one of the auxiliary or observational theories involved in the prediction. Thus given the Duhem-Quine and Kuhn-Feyerabend theses, anomalies are not grounds for

considering a theory refuted. Nevertheless, such anomalies are a more serious cognitive liability for a theory than a merely unexplained event. Anomalous events, unlike unexplained ones, are those for which the nonreportive expectations deduced from our explanatory theories contradict the reportive expectations given by using our observational theories. They indicate that our system of theories as a whole is inconsistent. Unlike anomalous events no such problem arises with unexplained events. In the latter case our theories are only unable to explain observed events that are considered to belong to their domain. Thus, where explained events provide evidence for our theories and unexplained events furnish no supporting evidence, anomalous events constitute evidence against our theories.

Therefore, any theory that can succeed in explaining events anomalous to a previously established theory merits serious consideration. In the first place, an event is anomalous to a theory precisely because no conceivable adjustment of the theory, its auxiliary theories or the observational theories was able to resolve this anomaly. To explain such an event we presumably need radically new assumptions that have, as yet, not been suggested. The new theory in showing that its assumptions can explain the anomalous event proves that these assumptions are not totally arbitrary.

Furthermore, one must remember that an event is anomalous to a theory only because it has not been possible to conceive any modification of the theory, or its auxiliary and observational theories, that can explain the event in question. This does not mean that there is no possible modification that can deal with the problem. The success of a new theory could turn out to be instructive. The new theory may have

isolated precisely those assumptions which, with appropriate modification, could be accommodated into the conceptual framework of its rival so as to enable it to explain these events. Thus, though a new theory may be extremely restricted in scope, it may turn out to be instrumental in expanding the domain of success of the older established theory. This is because it may indicate the kind of direction in which to develop an earlier theory so as to enable it to deal with problems that previously appeared anomalous to it.

Secondly, the new theory may be used to successfully predict observations that are anomalous to the old theory. In this case the new theory would merit even more serious consideration. For this would show that it is capable of generating fresh evidence against an established rival. Any theory that does this enhances its admissability even if it is only because of the heuristic role it can play in testing the limits of its rival.

Thirdly, it is important to note that by explaining events that are anomalous to the earlier theory, the new theory uses as evidence for itself what is counterevidence against its rival. If the new theory could be systematically developed to generate more anomalies against its rival, it could widen its base of experimental support through the same process that weakens the support for its rival. Thus, by presenting or explaining anomalies to the older theory, the new theory can widen its domain of success whilst simultaneously exposing the limits of its rival. This is because it is more unlikely that an older rival would be able to resolve an anomaly to itself, by some strategic modification, than it is likely to resolve what may have been merely a nonanomalous unexplained event.

Finally, the fact that the new theory can predict anomalies for its rival does not mean that the older theory cannot also be used to predict events that are anomalous to the new theory. Generally, when two theories are in competition, both may be used to generate anomalies for the other. In this way the competition between them is a simultaneous testing of the limits of both theories. In an attempt to respond to the problems revealed by their competitor, both theories would grow and evolve.

Thus the strategy of proliferation of theories that has been recommended by philosophers like Popper, Lakatos and Feyerabend is essential for the development of science.³⁸ One may understand how it would operate in a concrete context as follows. Consider a highly successful theory. This theory would generally confront a large number of unexplained events - including anomalous ones. Thus there would always be a number of rival mini-theories, whose content is much smaller, which succeed in explaining some of these events. This interaction is precisely the process through which the theory grows. For each of these theories not only reveals the limits of its rival but it also suggests the direction in which it, or its auxiliary or observational theories, could be adjusted to deal with presently unexplained events. Thus, even though the new theories may threaten their established rival they also provide the stimulus for its growth. Of course, there is always the possibility that one or more of these theories may expand their domain of success in such a way as to weaken, and ultimately replace, the earlier theory.

(b) Rational Preference

The notion of relative indispensability also suggests a criterion for rationally comparing the performance of two theories. Consider two rival theories T_1 and T_2 . Suppose that T_2 is such that (in conjunction with compatible auxiliary and observational theories) it explains all the events that are also explained by T_1 , but the reverse situation does not obtain. Then T_1 becomes dispensable since it cannot explain any event that its rival cannot. (In fact, if T_1 were a new theory it would not be considered admissible given the relative indispensability criterion for admission). The relative indispensability of T_2 with respect to T_1 is infinite, and the empirical content of T_2 includes that of T_1 . The theory T_2 is clearly preferable to T_1 , and the latter can be considered effectively eliminated. This is a situation in which the domain of T_2 subsumes that of T_1 - this is how the traditional view holds that theories get eliminated.

However, T_1 may also be eliminated without its domain being subsumed by a single successor theory. This occurs when the theories of the T_2 -complex divide the domain of T_1 among themselves so that, even if no theory of the new complex subsumes the domain of T_1 , the theories of the complex as a whole explain all the events explained by T_1 . Again the relative explanatory indispensability of T_2 with respect to T_1 is infinite, and T_1 can be deemed eliminated. In both the cases we have considered T_2 is clearly preferable to T_1 .

However, there may be situations where neither theory can be said to have eliminated the other. This happens when each theory can explain some events that lie outside the alternative theory-complex. In this case let us say that the preferred theory is the one whose scope outside

the alternative theory-complex is the greater. Or, in short, a theory T_2 is preferred to a theory T_1 if its relative explanatory indispensability with respect to T_1 is greater than one. Clearly, the larger the relative indispensability is the greater the degree of preference we will have for T_2 , since its scope outside the T_1 -complex will increase relative to the scope of T_1 outside the T_2 -complex.

Such a conclusion would be extremely counter-intuitive within the traditional framework. For it tells us that a theory T_2 may be preferable to a rival T_1 even if its absolute scope is much smaller than its rival. Thus even a theory which has greater generality, predictive power and explanatory power in absolute terms may be deemed to be less preferable. For what is important in determining its preference is not its scope, but its scope outside the rival theory-complex. Such a conclusion is very reasonable because, if the domain of a theory can be split by the theories of a rival complex, and the new complex of theories (as a whole) has more scope than the single theory, there is no reason to prefer the single theory merely because it has a larger scope than any single theory of the rival complex.

However, it does not always occur that two theories that start as rivals also end as rivals. It may turn out that the domain of a theory T_1 , that was initially a rival to T_2 , may be restricted so as to exclude the part of the domain in which it was in competition with T_2 . Then T_1 would remain a successful theory within this circumscribed domain whilst ceasing to compete with T_2 . The two theories may become allies with each handling a separate domain of phenomena. If we now attempt to compare their relative explanatory indispensability we get

the scope of either theory outside the other theory-complex as zero, so that the relative explanatory indispensability becomes an indefinite ratio of zero dividing zero. They are noncomparable since they are not in competition.

Examples where this has happened abound in science. Take the wave theory of light proposed by Young and Fresnel. The dominant theory in the field before the wave theory was Newton's corpuscular theory of light. The corpuscular hypothesis was considered to apply to all matter physical bodies as well as radiation. Thus Newton's theory of gravitation and mechanics was considered applicable to corpuscles of light. Problems like the mass of light corpuscles, the pressure of light radiation on bodies, and the effect of gravity on light were considered important.³⁹ After the success of the wave theory the corpuscular theory was not discarded. Instead its domain was restricted to physical bodies only, so that the corpuscular theory of matter and the wave theory of light ceased to be competitors. They became two theories that applied to different domains of phenomena - one dealing with physical bodies and the other with radiation. A similar change occurred when mechanical processes were sharply distinguished from electromagnetic ones by Lorentz. Maxwell had sought to subsume electromagnetic phenomena under mechanical ones by developing mechanical models of the ether. This attempt had led to no success. One of the great achievements of the Lorentz theory was to restrict the domain of mechanics in such a way as to exclude electrical and magnetic phenomena from it. Thus he introduced the idea that Maxwell's theory was not merely a further elaboration of Newton's mechanical views, but an entirely new model of the world that applied to a separate domain of events.⁴⁰

Clearly the notion of epistemic preference admits of degrees. The more the measure of relative indispensability a theory possesses with respect to another, the greater the degree of epistemic preference we accord it. Furthermore, there is a continuum from admissibility to preferability. For a theory to be admissible all that is required is that its relative indispensability be nonzero with respect to all other admitted theories. For a theory to be preferable to another this value has to be greater than one. But a theory whose relative indispensability with respect to another is, say, 1.5 is only marginally preferable. On the other hand, if a theory has relative indispensability greater than 100, say, we would be prepared to say that it is superlatively preferable to its rival. This does not mean that the rival is eliminated, but it certainly suggests that the rival is comparatively weak. What is the point at which we would be prepared to say that we accept a theory? Acceptance is a much stronger epistemic commitment to a theory than preference. Let us examine this issue.

(c) Rational Acceptance

Clearly one of the requirements for accepting a theory is that it be preferable over its rivals. Nevertheless, marginal preferability is not adequate grounds for acceptance. For acceptance to occur we have to presume that a theory has to be superlatively preferable over its rivals. The fact that a theory is superlatively preferable over some of its rivals cannot be grounds for acceptance, either. To be acceptable a theory has to be clearly preferable over all its rivals. Thus we may say that a theory is acceptable only when it is superlatively preferable to any other rival theory. Thus a theory with relative indispensability

greater than 100 with respect to all rival theories can be deemed acceptable, but not one that has a relative indispensability greater than 1.5 with respect to all its rivals. Though it is clearly preferable to all rivals its degree of preference cannot be deemed sufficient to win acceptance.

The notion of acceptance, too, can be said to have degrees like that of preference. It would even be possible to refuse to talk of acceptance per se: one may merely specify the degree to which a theory may be accepted in terms of its relative indispensability with respect to its strongest rival. Thus all acceptance would involve the notion of degree, and total acceptance would only occur when a theory has infinite relative indispensability with respect to all its rivals - i.e., it has no rivals that have explained events it has not explained. However, I shall not follow this route; instead I shall assume that there is some high value of relative indispensability which causes mere preference to become acceptance.

Our view of how theories win acceptance by superlatively increasing their relative indispensability with respect to rival theories enables us to comprehend more clearly the distinction between normal science and revolutionary science emphasized by Kuhn. Normal science refers to those periods when a domain of scientific research is under the hegemony of a single accepted theory. This does not mean that the theory does not confront competitors. There will always be admissible mini-theories that attempt to deal with some observations that are not accommodated into the accepted theory, but none of these competitors are sufficiently successful to win acceptance for themselves. Nevertheless, they will remain competitors so long as they have not been eliminated, either by

the major theory, or some other mini-theory. Even where further elaborations of the major accepted theory do manage to eliminate some competitors it is never without rivals, because new mini-theories are proposed all the time to deal with new events that are unexplained by it. Thus the period of normal science actually involves a single accepted theory in competition with an array of changing mini-theories.

Scientific revolutions occur when a rival mini-theory succeeds in developing in such a way that, in conjunction with a network of other theories, it manages to win acceptance for itself. This does not occur instantaneously. At first the new theory only manages to win admission by successfully dealing with a small array of problems. As its success increases it begins to become more and more relatively indispensable with respect to its major rival. There may be a point where its success becomes greater enough that the older theory cannot be deemed rationally acceptable - all we can say is that it is still rationally preferable to the new theory. The step towards acceptance proceeds through a stage where the new theory becomes preferable, and then superlatively preferable, to its earlier rival. When it wins acceptance we can say that a scientific revolution has occurred. In the same process, the other theories belonging to the new theory-complex may also win acceptance against those auxiliary and observational theories exclusively associated with its older rival. The result is that one network of theories is replaced by a completely different one. This is a conceptual change of considerable dimensions.⁴¹

Such a change can be viewed as a gestalt realignment of the way we confront the world. This is because the change in theories also involves a change in the way we structure experience - and even

experience the world. For the new theories when used as observational instruments provide a new experience, and a new way of formulating observation reports. Thus they divide up the world of events in a radically different way from that of their predecessors. What can such a change of experience, observation reports, theories and domains be but a transformation of the way we confront reality?

Changes of this sort are a standard, if infrequent, feature of science. A scientific revolution occurred when Lorentz's electron theory was replaced by the modern theories of quantum physics, relativity and elementary particles. A similar revolutionary change was involved when the classical caloric theory was superseded by the kinetic and ether theories of heat in the early nineteenth century. That part of the domain of the caloric theory concerned with processes of heat in matter was appropriated by the kinetic theory; and the problems of radiant heat were dealt with by the ether theories. In fact, with the discovery of infrared light, radiant heat was included under the wave theory. Dalton's revolutionary chemistry also brought about a distinction in the domain of preceding chemical theories. Where the latter had not distinguished between mixtures and compounds, the Daltonian revolution explained compounds in terms of a theory of atomic combination, whereas mixtures were dealt with by theories of physical chemistry. The reclassification in biology introduced by Linnaeus led to whales and dolphins being transferred from piscine to mammalian anatomy and physiology. The Copernican revolution led to similar changes on a vast scale. Aristotelian dynamics and optics had to be completely replaced by radically different ones to accommodate the theory. Even events considered to belong to meteorological theories -

like comets - came to be included in astronomical theories.

The notion of relative explanatory indispensability can now be seen to account for why scientists have deemed norms like generality, explanatory and predictive scope to be important. This is because the relative indispensability involves the scope of a theory that lies outside the scope of the theory-complex associated with its competitor. It is usually the case that the scope that lies outside the competing complex's scope increases as the scope of the theory increases. However, this need not always occur. There may be situations where the scope of the theory may increase relative to another, but the scope outside the competitor's associated theory-complex may decrease; its rival develops more powerful auxiliary theories. Thus even if it is a useful heuristic principle for scientists to develop more general theories, or theories with greater predictive or explanatory power, it is not a strategy that is guaranteed to lead to success.

This also reveals the significance of generating counterevident phenomena to competitors. Theories that do this not only guard against elimination by competitors, but they also ensure that they increase their scope outside the rival theory-complex. Thus they increase their relative indispensability with respect to their rivals.

Finally, I want to consider the interesting situation where two different theories T and T^* are such that all the events explained by one of them is also explained by the alternative theory-complex. Thus the scope of each theory is included in the scope of the competing complex. In this case the relative indispensability of T with respect to T^* become indefinite (zero divided by zero). This could happen even when both theories are so highly successful that they have to be deemed

acceptable when compared to all other rival theories. This is the case, e.g., with Heisenberg's matrix and Shrodinger's wave mechanics. It was also the case with Copernicus' and Brahe's theory of the universe in the sixteenth century.

The possibility that there can be more than one rationally acceptable theory has far reaching implications for the way we understand truth and the way theories relate to the world. As we shall see later, to say that a theory is true is to say that it is rationally acceptable in an idealized sense - it must possess, and continue to possess, an infinite degree of relative indispensability with respect to all its competitors. There may turn out to be more than one theory with this property. Each of these cannot be said to be better than the other, because their relative indispensability with respect to one another is indefinite. They are different true theories of the world. We shall see that such a possibility requires us to relinquish the correspondence theory of truth, and also the view that the entities postulated by true scientific theories exist independently of the perspective offered by the theories.

Some philosophers have argued that since there are losses when one accepted theory replaces another there is no scientific progress.⁴² There are losses because some events explained by a predecessor are not explained by the replacing theory-complex. This will not happen if the older theory is eliminated; it will happen if it has been gradually weakened till a new acceptable theory emerges. In this case the new theory (or theories) is accepted because its relative indispensability is large compared to the earlier theory. The older theory is still indispensable to explain some events not accounted for by the new

theory. This does not mean, however, that scientific progress has not occurred. There has been progress: the new theory gets accepted only because its scope outside the older complex is much greater than the scope of the older theory outside the new complex. Thus the new complex explains more events than its predecessor.

4.5 The Metamorphic Development of Science

The atomic and holistic empiricists present us with two different images of the growth of science. Neither accords with the development of science as it is historically revealed. The first perceives scientific development as cumulative. Scientific revolutions are those rare episodes that result in a dramatic accretive increase of facts; a widening of our experience of the world, and an extension of the domain of events explained by theories; succeeding theories subsume the domain of earlier ones. The holistic empiricists see science as cumulative in this sense only when it grows within a tradition. However, revolutions are non-cumulative disjunctive episodes in which the postrevolutionary science confronts a new experience of the world, new facts and new domains that are radically different from those of prerevolutionary science. The two sciences cannot be compared because language and experience have mutated to such an extent that they are incommensurable. The basic disagreement between atomic and holistic empiricists is with respect to the nature of revolutionary, rather than traditional (normal), science. For atomic and holistic empiricists normal science is cumulative; but revolutionary science is perceived as cumulative by the former, and disjunctive by the latter. Disjunctive revolutions do not involve progress - if progress has to be measured by some criteria that

enable us to compare pre- and post-revolutionary science. For the holists the standards employed to compare different theories are offered along with the world-views associated with them. By their respective standards each theory would be deemed preferable to the other, but there is no theory-transcending method to compare pre- and post-revolutionary sciences.

Both these accounts of scientific revolutions are insensitive to the subtlety of the relationship that exists between pre- and post-revolutionary scientific theories. They tendentiously emphasize relationships that do occur, but not in the either-or sense they offer. A more careful examination reveals that some experiences of pre-revolutionary science are retained in the post-revolutionary one whilst others are discarded; some facts are common but others are different; new theories include some of the presuppositions of earlier ones and exclude others. Even in meaning changes terms have commensurable shared meaning as well as incommensurable different meaning. Thus, in a sense, it is correct to view the change as a non-cumulative transformation, but it is not a change that does not offer some retention of previous knowledge. The revolution is actually a metamorphosis of previous knowledge: the material that was originally present in pre-revolutionary science is offered in a transformed manner in post-revolutionary science. Like the gestalt figure that is seen in different ways, but is nevertheless recognized to be the same figure, a metamorphosis occurs in the way we experience the world, represent facts, structure domains and conceptualize the world. Revolutions have cumulative and disjunctive features. They are neither only cumulative nor only disjunctive. They are metamorphic.⁴³

How do we isolate the cumulative features from the disjunctive ones? We have already seen how this can be done - by an appeal to the background equivocating language of the pre- and post-revolutionary sciences. This language offers meaning to terms that is common to both frameworks. The experience obtained through BEL is the core common experience; the reports in BEL constitute the facts that are common to both theories, and the statements that can be formulated in BEL give the theoretical presuppositions retained through the revolution. However, there is also a disjunctive dimension - meanings, experiences, facts and statements that are not a part of BEL are different for the two theories. Since the metamorphic revolution involves both cumulative and disjunctive features neither a totally relativist account, nor a totally absolutist one, can adequately represent it. Because BEL is constructed out of the pre- and post-revolutionary conceptual frameworks there is no need to appeal to a frame-transcendent language, or experience, to recognize the cumulative features; nor is there any need to view the change as totally cumulative - the disjunctive features are precisely those excluded from BEL and the BEL experience.

Neither the atomic nor holistic empiricist accounts enable us to understand the dynamics of scientific revolutions. For cumulative revolutionists the pre-revolutionary theories play no direct role in achieving the post-revolutionary science. Their foundationalist view refers to facts given independently of theories - hence, Einsteinian science, say, could have been discovered without going through the Newtonian phase, if the facts had been available in the seventeenth century. There is no reason to suppose that earlier theories play a role in offerring the evidence that subsequently leads to their

abandonment. Even the disjunctive view of revolutions does not offer the possibility of understanding the process of transition. New theories simply arise; they do not have any connection with earlier ones. They involve a new way of perceiving the world, but are not in any sense born out of previous ways of experiencing the world. The metamorphic view, that there are shared and incommensurable features of pre- and post-revolutionary science, allows us to recognize that science grows by a bootstrapping process - older theories are used as observational theories to furnish the evidence to support new explanatory theories. These new explanatory theories themselves may cause us to modify earlier observational theories (as intertheoretic competition forces us to adapt to a changing background equivocating language). Even though we use experience to support our theories, we also use theories to transform our experience. Cumulative revolutions do not affect experience; disjunctive revolutions merely replace one experience by another. Seeing revolutions as metamorphic allows us to recognize that we use experience to guide us towards theories that can, subsequently, be employed to transform our experience.

This bootstrapping method of developing science is achieved through the active interaction and competition amongst different theories. The notion of cumulative revolutions does not allow us to see the role of interaction, or even competition, in the development of theories. Theories replace each other but do not affect one another's development. The notion of disjunctive revolutions enables us to recognize competition, but cannot use this insight to explain how competition can allow theories to use one another in their development. This is because the competing theories do not have any element in common. But often

theories develop by internalizing the achievements of their competitors. Such internalization is possible only because there is a background, equivocating language that can mediate commensuration between theories. Thus, the relationships between competing theories can be fruitful for all of them, as each helps the others to achieve greater articulation.

4.6 Conclusion

We have seen that, given the Duhem-Quine and the Kuhn-Feyerabend theses, scientific explanation can only be adequately described in terms of a dialectical model. This model shows that there are no given scientific domains - these get specified only along with the theories that have been developed to successfully explain events in them. Thus, the view that the development of science involves the subsumption of the domain of an earlier theory by a successor one has to be rejected. Instead, we have to allow that the domain of a theory may be divided by a new complex of successor theories. This feature of theory replacement requires us to relinquish norms like generality, predictive and explanatory power of theories as crucial to theory comparison. Instead, theories have to be compared in terms of their relative explanatory indispensability with respect to one another. The measure of a theory's relative explanatory indispensability enables us to decide when it is admissible; preferable to others; and can be considered to be acceptable. It also shows that scientific revolutions cannot be interpreted as either cumulative or disjunctive episodes - they are metamorphic changes in which pre- and post-revolutionary theories exhibit commensurable and incommensurable features.

CHAPTER FIVE

PARADIGMS AND THE SOCIOLOGY OF KNOWLEDGE

5.1 Science and Global Theories

The rôle of global theories in science has only recently been recognized. They have been referred to by various writers as paradigms, research programs or research traditions. In this chapter I shall examine the nature of global theories. We shall see that they can be understood as providing scientists with an array of deeply held metâexpectations which can be articulated more specifically into scientific theories. The identification of such global theories also enables us to comprehend in greater detail the way in which sociocultural factors are constitutive of scientific theories, and a means of reconciling both externalist and internalist approaches to science as different perspectives on the content of scientific theories.

(a) Kuhn and Paradigms

Kuhn was one of the first philosophers to emphasize the significant role played by very general methodological and metaphysical commitments in the evolution of science.¹ Such commitments constitute a nexus of values and beliefs which are adopted by a community of scientists, and which are subsequently articulated into theories that could be used to explain different domains of phenomena. Many writers have argued that the notion of paradigm employed by Kuhn is vague and imprecise;² even Kuhn himself has subsequently agreed with his critics that his use of

the term conflated a number of meanings.³ This vagueness of the notion makes it difficult to isolate the meaning that Kuhn intends to convey, but there is no denying the heuristic role this notion has played in expanding our understanding of many areas of human knowledge.⁴ The most important dimension of Kuhn's notion of paradigm is his identification of it as a unit of science that is more global than any specific theory, even though paradigms are often introduced along with specific theories.⁵

Kuhn viewed every mature science as constituted by a single paradigm. E.g., electricity did not acquire scientific status until Franklin proposed its first universally accepted paradigm.⁶ The acceptance of such a paradigm allows scientists to develop a growing body of knowledge by developing theories along lines suggested by the paradigm. They are able to do this in a cumulative manner because their shared paradigm enables them to have shared standards, problems and solutions that give them time to indulge in more elaborate and detailed research.⁷

However, the development of a body of knowledge within the framework of a paradigm may lead to problems that cannot be solved within that framework. Such problems constitute anomalies and, though all paradigms confront some anomalies at any time in their history, during crisis situations these may mount astronomically. During such periods, and for Kuhn only during such periods, the scientific community begins to reexamine critically the fundamental commitments involved in their adherence to the paradigm. In their attempt to resolve the problems that are raised they gradually weaken and modify some of these presuppositions and look actively for alternative approaches. Such a

period of anarchy only comes to a close with the emergence of a new paradigm that provides a new constellation of commitments, and a period of normal science resumes. In the process there has occurred a radical change in the way scientists interpret facts, in the theories they have adopted, and in the sort of standards they employ to evaluate theories. This gestalt realignment requires us to suppose that the change from one paradigm to another involves a scientific revolution. Thus the growth of science involves periods of evolution, under the hegemony of a single paradigm, punctuated by periods of revolution involving paradigm change.

In spite of many of the defects in his interpretation, one cannot overemphasize the seminal contribution made by Kuhn in revealing the significant role of paradigms in the history of science. This can be seen most clearly when one contrasts Kuhn's view with the traditional logical empiricist one. The traditional view, with its emphatic attempt to purge science of metaphysics, perceives commitment to a paradigm as essentially a fall from scientific grace. Extratheoretical global commitments are acknowledged to play a role in the context of discovery, but even there they are only tolerated provided they are kept out of the context of justification.⁸ Thus Kuhn, in emphasizing the role of paradigms in the development of theories, has introduced a radically new dimension into the philosophy of science that has been proscribed by the standard view.

Furthermore, Kuhn is also one of the first philosophers to examine the role of such paradigms as constitutive of theories, and the mechanics through which one paradigm replaces another. His answers to both these issues have been criticized by subsequent writers, but only because they attempt to answer the same questions in a different way.⁹

Of greatest importance perhaps is Kuhn's analysis of the way sociocultural factors affect the content of paradigms, thereby providing a link between the nature of scientific theories and the cultural context in which they occur.¹⁰ Within the logicist tradition the emphasis is to view cultural influences as either absent, or as influences that have to be eliminated as far as possible in the interests of scientific objectivity.

Thus Kuhn's analysis not only reveals the existence of global commitments in science, but it also attempts to provide answers to questions like: How is a paradigm related to its constituent theories? How do paradigms evolve? Why do paradigms change? How are paradigms and paradigm changes affected by sociocultural factors? Though many of the answers provided by Kuhn have been subjected to criticism, some of his critics have responded by providing alternative answers to the questions raised. Of special importance are the views of Lakatos and Laudan each of whom develops in succession Kuhn's notion of paradigm in a manner more adapted to accommodate it into an explicit philosophy of science.

In spite of the significance of his notion of global theories, Kuhn's account of them is inadequate for various reasons. In the first place his notion of a paradigm is extremely vague. Kuhn himself seems to endorse this defect as necessary. He argues that although scientists can agree on the identification of a paradigm they cannot produce a full interpretation or rationalization of it. He suggests that scientists can agree that a Newton, Lavoisier, Maxwell and Einstein each produced a permanent solution to a group of problems; they cannot agree, or sometimes even be aware of, the abstract characteristics that make these

solutions permanent. He even suggests that there may not be an "underlying body of rules and assumptions that additional historical and philosophical investigation might uncover."¹¹ Using the analogy made famous by Wittgenstein, he suggests that the resemblance between various research problems and techniques that are adhered to by the members of a paradigmatic community may bear only a family resemblance (in the way 'games' or 'chairs' do).¹²

It seems to me that Kuhn, by attaching too much weight to the difficulties involved in specifying a paradigm, has concluded hastily that they cannot be explicitly interpreted. In the first place, if a paradigm cannot be interpreted explicitly, how do we distinguish the paradigm offered by a Newton, Lavoisier or Maxwell from the specific theory they presented embodying this paradigm? In the case of chairs and games, we are given many examples before being able to identify a new game or chair as having a family resemblance to the ones we have already been acquainted with. But Newton and Lavoisier did not present a family of theories - they each presented a single theory. Thus one cannot suppose that other alternatives to these theories - e.g., Laplace's theory or Dalton's theory - belonged to the same paradigm, unless we can isolate those features of the original theory that were paradigmatic. It is true - as Kuhn argues - scientists do not separate the paradigmatically significant components of their theory from the more specific components at their inception. It is also true that sometimes it is difficult to isolate the significant dimensions of a theory. Nevertheless, this does not mean that we cannot recognize the significant commitments of a Ptolemaic astronomer from a Copernican one, a wave theory from a corpuscular theory, Lavoisier's chemistry from

phlogiston chemistry, or Einstein's theory from Newton's.

Secondly, Kuhn's notion of a paradigm is too multifaceted to be usefully employed in understanding science. Though readers generally suppose that it refers to global commitments, Kuhn also subsumes under this notion a lot of other features of science that mask this significant dimension. Masterman has identified twenty one different ways in which Kuhn employs this notion. A paradigm could refer to laws, theories, instrumentation, a philosophy, a universally recognized scientific achievement or even a myth.¹³ Kuhn masks the significance of the notion he has introduced by "inflating the definition of a paradigm until the term becomes so vague and ambiguous that it cannot easily be withheld, so general that it cannot easily be applied, so mysterious that it cannot help explain and so misleading that it is a positive hinderance to the understanding of some central aspects of science".¹⁴ In fact, it is only after we eliminate this blanket use of the term to cover all things that we can provide a positive function for Kuhn's notion of global commitments.

His view that paradigms are vague and multifaceted leads Kuhn to hold that paradigms cannot be explicitly formulated. Thus paradigms are always implicit and never fully articulated. This, however, does not account for the fact that often scientists are well-aware that they are in conflict with paradigm induced commitments when they formulate certain theories. E.g., Newton was aware that his theory of action-at-a-distance violated the Cartesian paradigm and, furthermore, that it did so on precisely this point. Similarly, Lamarck's theory was not popular in the 19th century because it ran counter to the mechanical nonteleological world view even before it was experimentally shown to be

untenable. The numerous controversies over whether theories should be field, phenomenalist or mechanical at the end of the 19th century were debates about paradigms and not about theories. Behaviouristic psychology was explicitly formulated as a paradigmatic program for psychology before its theories were developed in any detail. In fact many of the controversies in science about methodological and ontological commitments cannot be understood unless we suppose that such commitments were openly formulated. For scientists can only debate about explicitly given assumptions.¹⁵

This is not to deny Kuhn's point that paradigms are often given implicitly by theories. E.g., Maxwell conceived of his theory as mechanical, though it was later used to found a field program for physics.¹⁶ Nor did Plank intend to introduce new paradigm into physics. But this only indicates that scientists are often not totally aware of the basic presuppositions in their theories, or where their theories may lead. The same phenomenon can be seen in geometry. When Saccheri developed a geometry without assuming the parallel postulate he was not aware that it was going to lead to a totally new geometry.¹⁷ The fact that theories often specify their paradigmatic core implicitly at their inception - a valid point of Kuhn's - cannot be grounds for supposing that this core must, necessarily, be implicit and not open to debate.

Another feature of Kuhn's notion of paradigms is that, at any one time, a mature science has only one such global theory. This is not historically supported. Interpreted normatively, it makes science excessively dogmatic.¹⁸ Interpreted descriptively, it does not accord with the history of science. It is not the case that Newton's theory

constituted a mono-paradigm for science immediately after it was formulated. It was not universally accepted until a hundred years later. What was accepted were different mechanical theories formulated in accordance with the Cartesian paradigm. The caloric and kinetic theories of heat were in competition for over a century. Similarly, before Darwin, there was a catastrophist and evolutionary model of biological evolution that had been in competition for a long time. Until a short time ago there were two major paradigms of cosmology - the steady state and the big bang theory. Often paradigms remain in competition for long periods, e.g., Ptolemaic and Aristarchian astronomy; wave and corpuscular theory of light; atomic and continuum theories of matter. Such periods are often characterized by tremendous advances in science.

For this reason, Feyerabend and Lakatos argue that science is not characterized by periods of normal science interspersed between periods of revolution, but by the copresence of conflicting paradigms.¹⁹ Furthermore, they also argue that it is precisely this competition between paradigms that fuels the progress of science. We have seen that the proliferation of alternative theories is the only way of testing the fundamental assumptions of any theory: a point that is also emphasized by Feyerabend.

Furthermore, if there are no competitor paradigms during periods of normal science, where do the alternatives come from during periods of crisis? How are such periods of crisis to be identified? If, as Kuhn supposes, paradigm changes are due to crises, and these crises can only be recognized in the context of other theories, we cannot expect there to be scientific revolutions only because a period of normal science has

run aground on critical problems. It must be because alternative paradigms have revealed critical problems to the original paradigm. This is because the inadequacies of one theory can only be revealed by contrasting it with the successes of another.²⁰

Kuhn's view that paradigms are given implicitly makes it impossible for him to provide any detailed link between a paradigm and the theory that is articulated from it. His position does not answer important questions like "Does a paradigm precede a theory or does a theory precede a paradigm?" In some respects Kuhn appears to imagine that paradigms are prior to their constituent theories. However he also seems to suggest that a paradigm can only be shown by giving a family of instantiating theories²¹ - his so-called exemplars. Any adequate theory of science would have to come to grips with this important issue.

Most writers - Kuhn, Lakatos, Laudan - agree that global theories are not directly testable. Their success depends on the success of the specific theories that are articulated out of them. Thus, if we are to comprehend paradigm changes as more than mysterious inexplicable conceptual reorientations, we have to understand how they can compete with one another. Since paradigm competition can only be mediated through the theories that instantiate them, we cannot get an adequate grasp of the problem until we understand the relationship between paradigms and theories.

The problem does not arise for Kuhn since he holds that communities that share different paradigms cannot communicate with one another. Their languages, problems and standards of evaluation are essentially different and, hence, even where they appear to discourse they are really at cross purposes. We have already seen that this is not the

case. Scientists who hold different paradigms can communicate, and they can compare their theories (even if the process through which they do this is quite different from that visualized in the traditional view).

One of the most important contributions made by Kuhn is his emphasis on the effect of sociocultural factors on the content of scientific paradigms. His historiographic study reveals very clearly that the culture of a scientific community affects the content of the scientific theories that are accepted therein. It reveals that science is as much a cultural enterprise as any other, and that one cannot ignore the social dimension in any attempt to understand scientific theories. Nevertheless, this raises the spectre of relativism that had confronted the early sociologists like Weber, Mannheim and Merton. If science is socioculturally determined in what sense does science provide us with objective knowledge? How can scientific knowledge be considered to progress if scientific theories change under the pressure of sociocultural factors? Nevertheless, there is no doubt about the significant thrust that Kuhn's work has given to the whole discipline of the sociology of knowledge.

(b) Lakatos and Research Programs

Lakatos' notion of research programs was, in one sense, a further step in the evolution of Kuhn's concept of paradigm, even though it was developed in opposition to Kuhn's views. Lakatos identifies a research program as being constituted by a negative and a positive heuristic, and a succession or sequence of theories. The negative heuristic (or hard-core) of the program is made up of a number of fundamental

assumptions that cannot be repudiated so long as one is working within the program. E.g., the negative heuristic of the Newtonian program was made up of Newton's three laws of motion and his theory of gravitation.²² The positive heuristic "consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research program, how to modify, sophisticate the 'refutable' protective belt."²³ The positive heuristic thus forms a protective belt around the negative heuristic, and suggests how theories may be modified to save the hard core in the face of anomalies. Such a positive heuristic may be formulated as a metaphysical principle.

Thus a research program for Lakatos is a series or sequence of theories that work within the network of assumptions allowed by the negative heuristic, and which are developed along the lines suggested by the positive heuristic. The continuity of the succession of theories constitutes the aspect emphasized by Kuhn as normal science,²⁴ and Lakatos' hard core is reminiscent of Kuhn's fundamental commitments offered by a paradigm. The positive heuristic guides the articulation of this paradigmatic core into specific theories.

For Lakatos the sequence of theories constituting a successful paradigm have to involve progressive problemshifts. If the research program is defined in terms of problemshifts - a sequence of theories T_1, T_2, \dots, T_n where each succeeding theory replaces its predecessor - then the problemshift is theoretically progressive if each theory has excess empirical content over its predecessor. It is empirically progressive if some of this excess content is corroborated. The problemshift is progressive if it is both theoretically and

empirically progressive. Otherwise, it is a degenerating problemshift.

Lakatos' notion of research programs can be considered, in many ways, to be an advance over Kuhn's notion of paradigms. In the first place, it is more explicit as to the nature of global theories. Hence, it accounts for how scientists are able to debate about the assumptions involved in such global theories. It also specifies how a global theory - or paradigm - can be connected to the specific theories that instantiate it. Furthermore, it allows for the possibility that there could be more than one research program attempting to explain the same domain of events. In fact, Lakatos encourages the proliferation of theories because competition will compel research programs to develop in ways that involve progressive problemshifts. Thus, in many ways, Lakatos' methodology involves a more precise articulation of the Kuhnian notion of paradigm.

Nevertheless, there are a number of inadequacies in Lakatos' conception of global theories. Lakatos does not tell us when a research program can be considered eliminated. He recognizes that a research program may appear to be degenerating for a while only to prove itself progressive later. Thus he cautions against the instantaneous elimination of a research program. He wants a theory to be judged only 'in the long run'. As Feyerabend has pointed out, such advice can be useful only if it is combined with a time limit. But such time limits can only be arbitrary - if a certain period of waiting is allowed, why not a longer period?²⁵ In the case of the example offered by Lakatos, Prout's hypothesis, the research program showed itself to be progressive only after a century. With this amount of time available, and with judgments being allowed only in hindsight, Lakatos' recommendations become

vacuous.²⁶

Secondly, Lakatos does not give any indication as to how different research programs are in competition with one another. Presumably any research program would be acceptable if it involves a sequence of progressive problemshifts. Nevertheless, it may be unacceptable precisely because it does not compare favourably with an alternative research program. E.g., Newtonian mechanics could have been developed in a progressive way even after the success of the Einsteinian program, for there is no reason to suppose that it could not develop new theories that are both empirically and theoretically progressive. In fact, but for the emergence of Einstein's program, it may even now be extended into new domains successfully. To resolve this problem, Lakatos has to tell us how theories that belong to different paradigms are in competition with one another. Such a suggestion is necessary because Lakatos supposes that programs can develop only if we allow for a proliferation of competing theories.

Thirdly, Lakatos allows only a limited number of ways in which theories which constitute a program can be altered. According to him each successor theory results from the addition of auxiliary clauses to its predecessor in order to accommodate some anomaly, or by semantic reinterpretations of the original theory.²⁷ As Laudan has pointed out,

this has the unfortunate consequence that two theories can belong to the same program only if one of them logically entails the other. However, the mini-theories that replace others belonging to the same global theory often involve the elimination of some assumptions. Thus successor theories need not entail their predecessors.²⁸

Furthermore, Lakatos' methodology of research programs ignores the role of sociocultural factors in affecting the constitution of global theories. This, in fact, is Lakatos' explicit goal, for he designed his methodology to exclude the significance of the cultural dimension emphasized by Kuhn. This is unfortunate. One of the most important aspects of Kuhn's notion of paradigm is that it is at the level of such max-theories that social and cultural factors play the most conspicuous role in science.

Finally, by overemphasizing the plurality of research programs Lakatos ignores the historical fact that at any one time there is often a dominant research program. Kuhn tended to emphasize the dominant paradigm as the only one; Lakatos tends to emphasize pluralism at the price of ignoring an important aspect of science that Kuhn has revealed. What is significantly shown in the history of science is that there are dominant paradigms that often co-exist in competition with a plurality of other paradigms.

(c) Laudan and Research Traditions

The concept of paradigms has been developed by Laudan more precisely and concisely than either Kuhn or Lakatos. In place of Kuhn's notion of paradigms, and Lakatos' notion of research programs, Laudan introduces the concept of research traditions. He defines a research tradition as follows:

A research tradition is a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain.²⁹

Thus, for Laudan, a research tradition is a network of ontological and methodological commitments.³⁰

A research tradition thus provides a matrix of guidelines for the construction of specific theories. The ontology gives in a general way the entities which exist in the domain of study and the way they interact. Part of the function of theories is to explain problems by 'reducing' them to the ontology of the research tradition. The methodology of the tradition indicates the legitimate modes of enquiry that are open to an investigator. To violate the methodological and/or metaphysical directives of a tradition is to repudiate the tradition itself.³¹

According to Laudan research traditions are not directly testable. In the first place, they are too general to be confronted directly with observational reports. In the second place, they contain normative elements that are not open to empirical tests. The success of a research tradition is, therefore, gauged by the success of the theories that exemplify it. A successful research tradition is one whose component theories lead to the solution of an increasing range of problems.

Laudan argues that the relationship between a research tradition and its theories cannot be one of entailment. This is because a research tradition only gives a network of general ontological presuppositions, and a general method of dealing with the problems in a domain. A theory, on the other hand, gives a very specific ontology and a specific set of testable laws of nature. Thus the tradition essentially functions as a constraint on the kind of theories that can be developed within a domain. Conversely, it may not be possible to deduce the research tradition merely by being given a theory (or theories) belonging to it.

Entailment cannot be the relation between a research tradition and its theory because a number of mutually inconsistent theories can belong to the same research tradition. Conversely, the same theory can appeal to different research traditions as its presuppositional base. E.g., within the Cartesian optical tradition there were those who argued that the velocity of light was greater in optically denser media, and others who denied that this was the case. Conversely, Newton's theory that light had certain periodic properties was accepted, not only by those who subscribed to the tradition of the wave theory, but also by those who subscribed to the corpuscular tradition.³²

Though Laudan's view constitutes a great advance over Kuhn's and Lakatos' notion of global theories, it too has a number of drawbacks. Most importantly, Laudan concedes that there are purely normative (i.e., methodological) components in his research traditions that cannot be empirically tested. He argues that normative components can only be indirectly tested by the adequacy of the theories that exemplify a research tradition. However, what is to prevent a research tradition from rejecting all theories that do not conform to its methodological prescriptions? How can prescriptions given by different paradigms be tested? It is not clear that they can be tested by the problem-solving effectiveness of their constituent theories. If the adequacy of a theory is to be judged by its problem solving effectiveness, as well as its conformity to these methodological prescriptions, what criteria are there to arbitrate when the methodological prescriptions conflict with the judgements given by problem solving effectiveness? If problem solving effectiveness is to be the ultimate criterion what need is there for such methodological prescriptions? Would they not be redundant?

In conjunction with Lakatos, Laudan emphasizes theoretical pluralism but ignores what has been noted by Kuhn - often there is one dominant paradigm in any domain of successful science. The history of science reveals that there are always a plurality of paradigms that compete, not only with one another, but also the dominant paradigm.

Finally, though Laudan does not completely ignore the way sociocultural factors are involved in constituting research traditions, he gives them only a minimal role. We shall deal with Laudan's views on this issue in greater detail later in this chapter. We shall see that his program for the sociology of science ignores the important dimension of science emphasized by Kuhn, but ignored in traditional empiricist philosophies.

5.2 Paradigms as Networks of Metaexpectations

We have seen that Laudan conceives of a research tradition as specifying an ontology and a methodology. This dichotomy between ontology and methodology is characteristic of many philosophies of science. Though philosophers recognize that the two are mutually dependent, they consider that there must also be a separation between them for the following reason. The ontology involves making assertions about the world - i.e., about the so-called objects of inquiry - whereas the methodology specifies the appropriate procedures that are to be used in investigating and explaining the behavior of the objects given in the ontology.

I want to argue that this dualism of ontology and methodology is a part of a particular philosophical doctrine. There is no need to presuppose this doctrine once we accept the expectational view of

statements. For, strictly speaking, the ontology is presented as a claim about how a domain of events in the world can be represented (e.g., in terms of matter and mind, as the interactions of corpuscular entities, or as exhibiting the action of certain types of causes). The methodology (be it inductivist, phenomenalist, operationalist or behaviourist) is given as a set of rules about how a domain of events should be represented. Nevertheless the ontological claims about how the world can be represented are also demands about how the world should be represented. Similarly, the methodological demands about how the world should be represented also presuppose that the world can be represented in the ways they specify. Thus both ontology and methodology provide us with a system of expectations which indicate, not only how the world can be represented, but also how it should be represented. In short, they furnish us with expectations about how events in the world are to be represented.

It is for this reason that ontology and methodology are inseparably interlinked. They are interconnected because they are different modes of specifying how we are to represent the events in a domain of inquiry. If we specify this mode of representation as if it was only about the events in the world - i.e., as a claim about these events - we obtain an ontological assertion. If we specify the mode of representation as a demand on how this should be done we obtain a methodological rule. Nevertheless, an ontological assertion is also an implicit demand because it is really an expectation. Similarly, a methodological rule, being an expectation, is also a implicit claim.

This means that all ontological claims can also be formulated as methodological rules, and all methodological rules can be formulated as

ontological claims. Popper has pointed out that some methodological rules may be formulated as metaphysical (ontological) principles.³³ Similarly Watkins³⁴ has shown how metaphysical (ontological) principles may be transformed into methodological ones. This possibility of transforming ontological principles into methodological ones, and vice versa, is possible only because both these principles are expectations. As we have seen expectations can be treated either as claims about the world (an ontological aspect) or as demands about the way the world should be represented (a methodological aspect).

Laudan specifies a research tradition (or paradigm) as a network of very general ontological and methodological principles. Since both these sorts of principles are expectations, and they are very general in nature, we may say that such principles are metaexpectations. Thus, we can define a paradigm as follows:-

A paradigm for a domain of inquiry is constituted by a network of metaexpectations for that domain of inquiry.

Such metaexpectations are ontological principles when they are treated as claims about the events in the domain of inquiry, and they are methodological principles when they are treated as demands about how events in that domain should be represented.

Let us examine this interpretation of paradigms to see if it is supported by the historical evidence. Consider the behaviourist school in psychology. Behaviourism was a new paradigm introduced by Watson in a reaction against the introspective psychologies of Titchener and James. It was a program designed to make the psychologist exclude 'from his scientific vocabulary all subjective terms such as sensation, perception, image, desire, purpose and even thinking and emotion as they are subjectively defined'. Instead the behaviourist seeks to describe

only behaviour, and this behaviour only in terms of stimulus and response.³⁵ Watson argued that the use of subjective language involving items in consciousness belonged to the days of superstition and magic, and a scientific psychology must purge itself of such concepts.

Watson's views were further developed by Skinner who argues in his influential work Science and Human Behaviour that 'mind' and 'ideas' are nonexistent entities 'invented for the sole purpose of providing spurious explanations.....Since mental and psychic events are asserted to take the dimensions of physical science, we have an additional reason for rejecting them'.³⁶ Behaviourism, as proposed by Watson, became one of the dominant paradigms in psychology for the next fifty years, and is still extremely influential.

Let us examine the metaexpectations embodied in the behaviourist paradigm. Its ontology asserts that the world does not contain the sort of events that earlier psychologists considered to specify their domain of inquiry - emotions, images, sensations, thoughts or even consciousness. Instead, psychology like the other natural sciences, confronts a domain of physical events. These events - insofar as they belong to organic behaviour - could be explained in terms of stimulus and response. Clearly this ontology could be interpreted as a methodology. A psychologist need not commit himself to the principle that there are no mental events, but merely allow that since mental events are inaccessible and private, all that scientific psychology can study is the stimulus-response behaviour of organisms. Thus behaviourism could be treated as offering a set of methodological principles rather than a network of ontological claims about the world. There are, in fact, many behaviourist psychologists who treat their paradigm as methodological

rather than ontological. Furthermore, there are some writers who have argued that behaviourism itself began as a program for a methodology that sought to exclude mental events as objects of study, but which was later transformed into an ontology that claimed that such phenomena did not exist.³⁷

The principles of behaviourism can be treated either as ontological or methodological precisely because they are metaexpectations. The paradigm offered is not purely an ontology, or purely a methodology, but a network of metaexpectations. There are other examples in the history of science where ontological principles have been transformed into (or treated as) methodological ones. The Aristotelian paradigm for explaining the motions of heavenly bodies supposed that they moved in circular orbits with uniform speed. For Aristotle this was an ontological claim; it was later transformed into a methodological principle by Ptolemy. Ptolemy accounted for planetary motions by supposing that they were compounded of uniform circular motions, but he viewed his theory as essentially involved in 'saving the appearances'. He was aware that more than one model could be constructed to give the orbits of the planets; and supposed that the account he gave was merely a mathematical device that need not describe the actual orbits in which the planets moved.³⁸ In interpreting his work in this fashion, Ptolemy set in motion a tradition that took the Aristotelian principle as methodological rather than ontological. Part of the debate between Galileo and Bellarmine in the 17th century was connected with the issue of whether the principle, that the sun was at the centre of the universe, was methodological or ontological. Bellarmine would have been satisfied if Galileo had interpreted the heliocentric principle as a methodological

rule to save the phenomena, but he insisted that as an ontological claim it was a violation of the scriptures.

Similarly, a methodology may sometimes be transformed into an ontology. At the end of the 19th century there was an attempt by philosophers and scientists like Mach, Poincare and Avenarius to purge from physics the notions of absolute space, absolute time and absolute motion. These fundamental concepts of classical physics were deemed to be unobservable and, therefore, unacceptable in a positive science. Originally it was advanced purely as a methodological principle but, with the rise of Einstein's theory, it has become an ontological principle of modern physics. Today it is assumed that there is no absolute space or time, and that the laws of physics have to be formulated in such a way that they are invariant in all inertial frames of reference. In fact, Einstein and others have attempted to extend the principle even to accelerated frames of reference (though this program has not met with complete success). Thus, the principle that only relative motions are significant may have begun as a methodological rule, but it is now treated as an ontological claim.

The more closely we examine the problem the more difficult it becomes to separate the methodological and ontological commitments embodied in a paradigm. What one scientist may treat as methodological another may consider ontological, and vice versa. This can be understood if we view the principles embodied in a paradigm as a network of metaexpectations. Charles Lyell's uniformitarian paradigm for geology supposed that only presently acting causes could be assumed to explain events in the past. Thus, it eliminated all catastrophist explanations of geological formations. Is this to be presumed as an

ontological claim about the causes that were active in the past, or a methodological demand on the kind of causes that are allowed in a scientific explanation? Cartesian physics required that all phenomena be explained by assuming particles that could only interact by direct contact. Thus, it precluded the possibility of invoking action-at-a-distance. Should this be deemed an ontological principle about the world or a methodological demand on the kind of theories that are allowed to explain events in the world? Similarly, the scientific veto on teleological explanations that made Darwin's mechanical theory so attractive in the 19th century - and to an extent even today - can be treated as a methodological rule or as an ontological principle.

It may be especially difficult to view methodological norms as expectations. To support this position let us examine some classes of methodological norms. Firstly, there are those methodological norms that specify the kind of concepts that are allowed (or excluded from) a theory. It may be required that the acceptable concepts are those which are observable, operationally definable, nonmentalistic and so on. Each of these norms are demands specifying the kind of concepts that are allowed in theories constructed to represent a domain of events. In short, they indicate what should be the acceptable ways of representing reality. Nevertheless this is also to presuppose that the events studied can be so represented. E.g., to say 'Only operationally definable concepts should be employed' is to presuppose that operationally definable concepts can be employed to represent the domain of events. If it is found that one cannot construct successful theories whose concepts are all operationally definable then the demand has to be dropped. Thus, what appears to be a purely methodological rule regarding concepts

is really the expectation that 'Operationally definable concepts can/should be used to represent events'.

Secondly, there are methodological rules that specify how acceptable theories are to be constructed. These rules may require that the principles of a theory must be intuitively self-evident; or that they must be induced from the phenomena; or that they be testable by means of hypothetico-deductive strategies. However, such methodological demands also presuppose that these are satisfactory methods to be employed to arrive at theories that can acceptably represent a domain of events. Thus, to prescribe that an acceptable theory should employ these methods is, also, to claim that an acceptable theory can be obtained by these methods. The world may be such that this is not possible. Thus the methodological principles offered also embody ontological content - i.e., they are really metaexpectations.

Consider the problem more closely. A world in which intuitive self-evidence guarantees truth is not logically inconceivable; yet we can say that Descartes was wrong in presupposing that the world we inhabit is such a world. To suppose that inductive methods are sufficient to lead to acceptable theories presumes, as we have seen, that the evidence from which induction begins is theoretically uncontaminated. This is untenable; and it is an empirical discovery that this is so. Similarly, a straightforward hypothetico-deductive account of theory-acceptance also presupposes that the data that scientific theories address are not theory-laden. Even a theory that has successfully explained all the events considered to belong to its domain may be rejected, because the phenomena it addressed are seen to be counterfactual. We may find grounds for rejecting the observational

theories employed to obtain them. Clearly, even inductive and hypothetico-deductive strategies (which appear to possess only methodological content) presume an ontological claim. They assume that our experience of the world is uncontaminated by theory (which itself, is a claim about the world).

Even the method of theory-evaluation that I have proposed - comparing theories in terms of their relative explanatory indispensability with respect to others - is not free of ontological commitments. If we deny that an external world exists independently of our theories and assume, instead, that different theories confront different worlds (which come into existence as a consequence of adopting these theories), such a methodology would be unacceptable. There would be no point in evaluating theories in terms of their explanatory indispensability since they would not be about the same world. Thus, the methodology of scientific rationality I have offered also embodies ontological claims. It presupposes a mind-independent external reality. Such an assumption is perfectly in accord with all that modern science has to say about the world; but it is a metaexpectation that is open to empirical disconfirmation.

The endeavour to create a scientific methodology transcendent to all ontological claims is a fruitless and futile quest - it is merely another attempt to ground science upon a foundational basis. Scientific methodologies themselves are open to dialectical and empirical testing in the way that scientific theories are. They are metatheories about the world - they involve metaexpectations.

Finally, there are those methodological rules that specify the sort of content that theories should have to be acceptable. E.g., they may

require that theories be deterministic, mechanistic, materialistic, predictive and so on. It should be clear from our previous discussion that such rules are metaexpectations. E.g., to say that a theory should be deterministic is to presuppose the claim that a deterministic theory can represent the domain of events. This may not be possible, as the quantum theory shows.

The intimate link between methodology and ontology that has been noticed by some philosophers is now clear. For when scientists and philosophers talk of methodology, they imagine they are referring to criteria as to how the events should be represented. When they talk of ontology they imagine they are referring to objects and their behaviour in the world. Actually all they have are representations of events in that domain. Ontological talk arises from the fact that, since the events can be represented in a particular way, the terms of the representation appear to refer to elements of reality. Methodological talk is about how the events should be represented. How these events ought to be represented and how they can be represented are mutually interdependent. This is because they are complementary aspects of expectations about the representation of these events.

Thus a paradigm is a network of metaexpectations. Those metaexpectations that are treated as being about the world (or the way the world can be represented) appear as ontological claims. Those that are treated as demands on how the world should be represented appear to be methodological principles. In the former case they appear to be descriptive; in the latter case they appear normative. Actually they are neither descriptive nor normative - they are metaexpectations. For this reason a metaexpectation that is treated as an ontological principle may also

be formulated as a methodological one; and vice versa.

It is now evident why paradigms have appeared to display both descriptive and normative behaviour to scientists. On the one hand, the metaexpectations they embody may be considered as demands on a scientific inquirer in the relevant field. On the other hand, these same metaexpectations may be deemed to give the most general description of the objects in the field of inquiry. Overemphasis on the normative aspect of paradigmatically given metaexpectations has lead some philosophers to assert that, since norms cannot be empirically tested, paradigm conflicts cannot be empirically resolved. This problem does not arise if the norms given by paradigms are really metaexpectations.

Nevertheless, it is not easy to test the metaexpectations given in a paradigm directly against experience. This is because their extreme generality prevents us from adopting such a straightforward procedure. Nevertheless they are indirectly testable. Paradigms are tested by means of theories that are articulated out of them. To deal with this problem I will now examine the connection between a paradigm and its constituent theories.

5.3 Paradigms and Theories

We have seen that paradigms are networks of metaexpectations. What is the relationship between paradigms and their constituent theories? I want to argue that theories are more specific articulations of these metaexpectations. E.g., the paradigm embodied in the kinetic theory of gases suggested that heat was due to the motion of atomic particles, and that a rise in temperature was due to the more rapid motion of such particles. However, this paradigm did not become a scientific theory

until a more specific formulation of it was achieved by Clausius. It was only thus that the paradigm was able to make contact with the results of observation.

In order to articulate the paradigm into a scientific theory, it was necessary for Clausius to make a whole network of more specific expectations about the properties and behaviour of the elementary particles. He had to assume that they were spherical, perfectly elastic and obeyed Newton's laws of motion. Furthermore, he had to assume that there was a large number of them in any small volume, and that their distribution in this volume was perfectly random. In addition, he had to suppose that there were no forces between these particles other than that of gravitation and, what gravitational attraction there was between them could be ignored. Only then could a theory be developed to explain the results of observation. Clausius used it to account for Boyles' law, Charles' law and the specific heat of gases. Thus, the extremely general metaexpectations provided by the kinetic program had to be complemented with a great many more specific expectations in order to achieve a testable scientific theory. Furthermore, it was the failure of the specific theory to account for all the events in its domain that lead to a series of modifications by Maxwell, Boltzmann and Einstein (all working within the kinetic paradigm), each improving the theory of their predecessor, to bring observed results in line with the metaexpectations of the kinetic paradigm.

Similarly, the heliocentric paradigm for the solar system proposed by Aristarchus in the 2nd century B.C., had to wait nearly 2000 years before it was articulated into a testable scientific theory. The theory proposed by Copernicus involved making a whole network of more specific

expectations about minor epicycles and equants to account for the observed motions of the planets.³⁹ Like the kinetic theory of Clausius, Copernicus' theory was modified by his successors. Kepler made the orbits of the planets elliptical with the sun at one focus, and Newton made the centre of gravity of the sun and the planets the point about which all the bodies of the solar system orbited. Nevertheless, each of these modifications did not involve a dramatic change of the paradigm introduced by Copernicus' specific theory - i.e., that the sun is essentially the centre around which the planets are in orbit.

The above examples make it clear that logically incompatible theories could belong to the same paradigm. Clausius' specific theory was incompatible with Maxwell's, and Maxwell's with Einstein's. Nevertheless, they belonged to the same paradigm because they shared its metaexpectations. Similarly, Copernicus' specific theory was logically and empirically inconsistent with Kepler's or Newton's theories, but all these theories embodied the metaexpectations of the paradigm introduced by Aristarchus.

Thus, as pointed out by Laudan,⁴⁰ the relationship between a paradigm and its constituent theories cannot be one of logical entailment. For if this were the case logically incompatible theories cannot belong to the same paradigm. However, it can be said that a paradigm that leads to a successful specific theory thereby obtains empirical support for the metaexpectations it embodies. This is because even if a theory is not logically derivable from these metaexpectations, it does presuppose the metaexpectations offered by the paradigm. E.g., every kinetic theory presupposes that matter is made up of corpuscular particles, and that it is the motion of these particles that is detected

as heat. In this respect it denies that heat is a substance - a view offered by the now defunct caloric paradigm. Every heliocentric theory assumes that the earth is in orbit around the sun, and thus denies the metaexpectations of any paradigm that proposes that the earth is stationary. Thus the success of any theory articulated out of a paradigm vindicates the metaexpectations of the paradigm. Let us refer to a theory that presupposes a paradigm as an instantiating theory of that paradigm. Thus:-

A theory instantiates a paradigm if its expectations presuppose the metaexpectations of the paradigm.

Let us examine more closely this relationship between a paradigm and its instantiating theories. Firstly, given a domain of events a paradigm may be instantiated by a sequence of theories each of which is incompatible with its predecessor, and is an improvement on its predecessor. This is reminiscent of Lakatos' notion of a research program as constituted by a sequence of theories, with each successor theory being empirically and theoretically progressive with respect to the one that temporally preceded it. However, Lakatos imposes requirements that make it necessary to suppose that a successor theory logically entails its predecessor. This, however, is not a requirement that is often fulfilled in the history of science. Kepler's theory was logically incompatible with Copernicus' even though they were instantiations of the same paradigm.

Secondly, a paradigm may be instantiated by a number of mutually coexisting and competing theories. A classic example of this is the wave and corpuscular theories of light. Both these theories belonged to the same mechanical paradigm, except that the corpuscular theory held light to be made up of particles whereas the wave theory envisaged light

as vibrations of an ether medium. The success of either of these theories would lend support to the mechanical paradigm. Similarly, the paradigm offered by the quantum theory has a number of competing instantiating theories - quantum field theories, S-matrix theories, group theories and renormalized field theories. The Darwinian paradigm of evolution by natural selection ~~may be articulated into a number of~~ competing new-Darwinian theories, and Freud's views have been developed in different directions by neo-Freudian schools. Clearly, the success of any one of these different instantiating theories of the paradigm would constitute a success for the paradigm itself.

Finally, a paradigm may be instantiated by a number of theories which are not competitors since they apply to different domains of phenomena. Because a paradigm is a set of very general expectations about the world, it may be open to articulation in many different directions. The mechanical paradigm formulated in the 17th century led to mechanical theories of matter, light, psychology and physiology.⁴¹ These instantiating theories were all different from one another, but were not competitors since their domains were mutually exclusive. Similarly, the evolutionary paradigm first formulated in the 19th century led to theories that sought to understand biological, psychological, sociological and even astronomical phenomena from the genetic and developmental point of view. The Darwinian theory of evolution, Freud's theories of developmental psychology, the historicist interpretations of society and the big-bang theory of the universe are all articulations of the evolutionary point of view. Paradigms like these, that can be articulated into instantiating theories in many areas of study, are extremely fruitful and often become constitutive of the

fundamental ways in which we view the world.

However, there are numerous instances where a theory that is seen to be compatible with a paradigm does not presuppose the metaexpectations of that paradigm. The wave theory of light and Maxwell's theory of electromagnetism neither supported nor reduced support for the Darwinian paradigm. Nevertheless, the issue may not be as simple as it appears. It was noticed by Kelvin⁴² in the 19th century that the laws of thermodynamics led to the prediction that the sun could not have existed more than a few tens of thousands of years. Nevertheless, Darwin's theory required that life should have existed on the earth for a much longer period of time to account for the evolution of organic species. Thus the theory of thermodynamics appeared to conflict with Darwin's theory, even though the expectations embodied in the thermodynamic theory neither presupposed, nor conflicted, with the metaexpectations of the Darwinian paradigm. However, this conflict was later seen to be due to an auxiliary theory that was used in conjunction with thermodynamics to make the prediction - namely that the source of the sun's energy was chemical reactions. Once it was recognized that the sun was a nuclear reactor in space the two theories became compatible. Thus, even if a theory leads to predictions that are incompatible with the predictions made by the instantiating theories of a paradigm, it is not sufficient grounds for supposing that the theory is incompatible with that paradigm. The discrepancy may be due to auxiliary theories that are involved in the predictions. Thus, regardless of whether a theory makes predictions that are incompatible with those of an instantiating theory a paradigm, we may say that it is commensurate with a paradigm if the following condition holds:-

A theory is commensurate with a paradigm if its expectations do not presuppose, but are compatible with, the metaexpectations of that paradigm.

However, there are situations where a theory may include expectations that violate the metaexpectations of a paradigm. In this case there is no possibility of supposing that the incompatibility between the predictions of the theory and that of the instantiating theories of the paradigm may be due to other auxiliary theories involved in the prediction. Einstein's invariance of light velocity postulate clearly violated Galilean kinematics and the Newtonian paradigm. Bohr's model of the hydrogen atom supposed that electrons in orbit did not radiate electromagnetic energy. This was in direct conflict with the Maxwellian paradigm. Similarly, the catastrophist theories in geology in the 19th century violated Lyell's uniformitarian paradigm. Theories which contain expectations that violate the metaexpectations of a paradigm may be said to be incommensurate with that paradigm. Thus:-

A theory is incommensurate with a paradigm if its expectations are logically incompatible with the metaexpectations of that paradigm.

It has to be emphasized that a theory is not incommensurate with a paradigm merely because it leads to predictions that are incompatible with the predictions of the instantiating theories of that paradigm. Such a conflict is not serious, because the blame may lie in the specific instantiating theories or in other auxiliary or observational theories. Thus, new developments may resolve the conflict. Furthermore, even within a single paradigm there may be conflicting theories. Thus there have been different mechanical theories of heat; there are conflicting neo-Darwinian theories of evolution; and mutually incompatible instantiating theories for the quantum paradigm. If

logically incompatible theories can belong to the same paradigm, the conflict between a theory and an instantiating theory of a paradigm cannot be reason for supposing that the theory in question is incommensurate to the paradigm. For a theory to be incommensurate with a paradigm, we require that its expectations should be directly incompatible with the metaexpectations of that paradigm.

We have seen that fruitful paradigms are those which lead to highly successful instantiating theories. We have considered a number of examples of these - the mechanical and evolutionary paradigms, the Freudian paradigm and Copernicus' heliocentric theory. On the other hand, there may be paradigms which have no successful instantiating theory. Let us refer to such paradigms as vacuous paradigms.

A paradigm is vacuous if there are no successful theories that presuppose the network of metaexpectations provided by that paradigm. (I.e., there is no successful instantiating theory of the paradigm).

A paradigm may be vacuous because all of its instantiating theories have been eliminated. The caloric paradigm of heat, that supposed heat to be a substance, was extremely successful in the 18th and early 19th centuries. It has since become vacuous. We may say the same for the four element theory of matter, Aristotelian dynamics, the theory of spontaneous generation, Ptolemy's geocentric theory and even Newtonian dynamics. On the other hand, a paradigm may be vacuous because it has never succeeded in leading us towards successful instantiating theories. Examples of these are the energeticist paradigm of Ostwald in the 19th century, the paradigm offered by psychometrics, and Lamarck's teleological paradigm of evolution.

5.4 Paradigm Competition and Scientific Revolutions

Let us now examine the problem of how paradigms compete, and what causes one paradigm to replace another. We have seen that the very general metaexpectations provided by a paradigm cannot be directly tested. The only way a paradigm can be tested is through its instantiating theories. A successful paradigm is one that leads us toward successful instantiating theories. We have already developed a model that gives us an account of how theories compete. We may now use it to understand paradigm competition.

Two paradigms are in scientific competition only if they have instantiating theories in competition. If they have no instantiating theories in competition we cannot suppose the paradigms to be in scientific competition even if they can be considered to be in competition as philosophical points of view. The heliocentric and geocentric paradigms were well known from the time of Aristarchus, but they entered into scientific competition only when Copernicus developed an instantiating theory of the heliocentric paradigm that was considered to be an admissible competitor to Ptolemy's theory. Furthermore, with the elimination of Ptolemy's theory, about the time of Newton, the paradigms ceased to be in competition, and the heliocentric paradigm had replaced the geocentric one. In the process the geocentric paradigm became vacuous because it ceased to possess a successful instantiating theory.

Kuhn has provided an image of science that is characterized by periods of evolution within a paradigm separated by periods of revolution when one paradigm replaces another. We have seen that this has been criticized by others like Lakatos, Feyerabend and Laudan who

argue, that at any one time, there are always a number of competing paradigms. Both points of view are supported by an appeal to the history of science.

However, a more careful examination of the historical data reveals that in a mature science we often recognize a dominant paradigm surrounded by a number of minor paradigms that are not as successful. Thus the dominant evolutionary paradigm of Darwin has always had to confront alternative neo-Lamarckian paradigms; the dominant corpuscular paradigm for light in the 17th century was always in competition with a minor school that traced its descent from Huyghen's wave theory. Similarly, the paradigm of Newtonian mechanical atomism was, throughout its history, in competition with a physics based purely on forces which traced its origins to Leibnitz, Kant, Boscovitch and was developed with great success by Faraday. Each of the competitor paradigms had instantiating theories that were successful in dealing with some domain of phenomena that appeared counterevident to the dominant paradigm.

During periods of scientific evolution the instantiating theories of the dominant paradigm are developed in such a way that they show themselves capable of dealing with larger and larger domains of phenomena. Thus there is a cumulative increase in the domain of success of the major paradigm. This does not mean that the minor paradigms are rendered vacuous in the process. For their instantiating theories may be successful in dealing with a certain domain of events that remain unexplained, or counterevident to, the dominant paradigm. In this process there may be a cumulative increase in the success of both the dominant paradigm and its rivals.

During periods of scientific revolution the instantiating theories of the minor paradigms may be developed in such a way that they become increasingly successful and may even end up by weakening or eliminating the instantiating theories of the dominant paradigm. As a consequence, the dominant paradigm becomes the minor paradigm or even vacuous, and a new paradigm attains the position of dominance.

Such a revolution is extremely significant for a number of reasons. In the first place, the metaexpectations embodied in paradigms are the most general expectations we have regarding reality. Hence, a change of paradigm involves a change in the most basic way we cognize reality. Secondly, these metaexpectations are both ontological and methodological. Thus a new paradigm not only indicates a way in which reality can be represented, but it also demands a new way in which reality should be represented. Thus it provides a new guideline for how the enterprise of science should be conducted in dealing with the domain of events that lie within the scope of the paradigm.

Furthermore, alternative theories are also alternative instruments through which we can view the world. The theories provided by the new paradigm can be used as observational theories through which to look at the world. Since our experience of the world is structured by the theories we use to observe the world, a change of paradigm involves a change in our experience of the world. Even if the world were to remain unchanged the way we confront it in experience has become transformed as a result of the change in paradigm. Such a change cannot be viewed as cumulative: it is a gestalt realignment of the way we both experience and represent the world.

5.5 The Sociology of Knowledge

We have seen that a paradigm is a network of metaexpectations that is tested through its instantiating theories. Let us now consider the problem of determining how the content of rationally accepted scientific theories is to be explained. There are two significant schools of philosophy that purport to deal with this issue. One holds the internalist view that the only explanation of the content of rationally acceptable theories is their capacity to explain the events in the relevant domain of inquiry. The world of events so restricts the content of rationally accepted scientific theories that external sociocultural factors have no scope to play in affecting their content. Thus the only explanation for the content of any rationally accepted theory is that it provides a rationally acceptable explanation of the world. Opposed to this view is the externalist school which holds that the content of theories is underdetermined by the events in the world; evaluations of rational acceptability of the content are not sufficient to explain the content. According to the externalists sociocultural factors are crucially involved in influencing the content of (even) rationally accepted theories of science.

Of course, the internalists allow that there still could be room for a sociology of knowledge, but it would be a noncognitive sociology. Such a sociology does not seek to account for the content of acceptable theories; it addresses itself to issues that relate to the genesis, evolution and demise of scientific institutions, the modes of organization of such institutions, how they affect the distribution of scientific knowledge, and so on. Furthermore, it may concern itself with the kind of sociocultural factors that determined why certain areas

were subjected to more intensive scientific studies than others; why certain scientific problems were treated as significant whilst others were ignored. There is no doubt about the significance of such studies but they essentially leave untouched the problem of explaining the content of rationally accepted theories. The internalists grant that the problems selected for scientific investigation may be socioculturally determined, but they would insist that the content of the theories themselves is not affected by the sociocultural milieu in which they arise. E.g., it may be claimed that sociocultural factors can account for why Maxwell's theory of electromagnetism was developed in England and not in Spain, but had the problems concerning electromagnetic phenomena been studied with the same zeal in the 19th century Spain, a Spaniard could have achieved the synthetic theory of electromagnetism, and it would have turned out to be the same theory as Maxwell's. Thus the content of rationally tested and accepted theories is solely determined by the empirical data, though sociocultural factors may play a role in determining which domains are subject to study.

Opposed to this internalist position is the claim that the content of even rationally accepted theories cannot be accounted for merely by the domain of events being investigated. It is argued that sociocultural factors play a crucial role in determining the very nature of scientific theories. The cultural context shapes the expectations embodied in theories in such a way that the content is, to a significant extent, determined by sociocultural influences. This is clearly a demand for a thoroughgoing cognitive sociology of knowledge. It is not merely the claim that the sociology of knowledge is concerned with the circumstances surrounding the production of knowledge, but that it has to explain the

content and nature of scientific knowledge.⁴³

The view that the internalist and externalist approaches are mutually incompatible is generally presupposed by most philosophers.

Laudan, who may be considered to represent a strong internalist position, asserts:-

"The intellectual historian of knowledge will generally seek to explain why some agent believed some theory by talking about the arguments and the evidence for and against the theory and its competitors. The cognitive sociologist of knowledge, on the other hand, will generally try to explain why the agent believed the theory in terms of the social, economic, psychological and institutional circumstances in which the agent found himself. Both are trying to solve the same problem (namely, the belief of some historical agent), yet their modes of solution are so different as to be also incommensurable."⁴⁴

Bloor, a staunch advocate of the externalist view endorses this:-

"The teleological (internal) and causal (external) models, then, present programmatic alternatives which quite exclude one another. Indeed, they are two opposed metaphysical standpoints... where objections and arguments are proposed against one of the two theories it will be found that they depend on and presuppose the other, and so beg the question at issue."⁴⁵

Contrary to what Laudan and Bloor suppose, the incommensurability or incompatibility of an internalist and externalist program is more apparent than real. Of course this would depend on the sort of programs we envisage - certain types of internal programs cannot be compatible with certain types of external ones. But there is no need for a theory of internal rationality, such as the one I have proposed in the last chapter, to be incompatible with an externalist account of the content of scientific theories. All that an internal theory of rational evaluation is required to do is to evaluate a theory as rationally acceptable in terms of its relative explanatory indispensability compared to other theories. However, such an evaluation does not

furnish us a rationally acceptable explanation of the content of the theory in question. The evaluation of a theory as acceptable is not sufficient to explain its content. The explanation of the content requires us to take into account sociocultural factors that are not considered when we evaluate a theory.

This is clear because a rational evaluation of the theory as acceptable only tells us that it is to be superlatively preferred to other theories in accounting for a domain of phenomena. We are still left with no explanation of the content of the theory itself. To evaluate the content of a theory as furnishing the best account of the domain of events of question is to tell us something about the content, but it is not to provide an explanation of the content. To assume that the content is explained the moment it is shown to provide the best explanation is similar to assuming that a racing car's design can be totally explained by showing that it is the best racing car there is. It is the best car because of its design, but its design is not totally explained by merely showing that it is the best racing car. Similarly, a theory provides a rationally acceptable explanation by virtue of its content, but its content is not explained by pointing to the fact that it provides such an explanation. The content of a theory, like the design of the car, is affected by the sociocultural context in which it arises.

There is no need for a thoroughgoing sociology of knowledge to deny that purely internal considerations are sufficient in evaluating a theory as rationally acceptable, provided it is allowed that this, in itself, is not sufficient to rationally account for the content of the theory. There is no need for a thoroughgoing internalist to deny that

sociocultural influences affect the content of a theory, provided it is allowed that one can determine internally whether a theory is rationally acceptable. For it is possible for both internalists and externalists to agree that the rational evaluation of a theory as acceptable does not offer a rational explanation of its content. A rational explanation of the content has to take into account sociocultural factors, but a rational evaluation need not.

The reason why the externalist and internalist positions appear irreconcilable is because debates about them always assume (sometimes tacitly) atomic empiricist foundationalism or holistic empiricist non-foundationalism. The former ties theories down to the world to such an extent that there is no scope left for external influences to affect their content. The latter detaches theories from the world in a way that precludes any possibility of a theory of internal rationality. All that it allows is a purely constructivist account of theories - one where only external (social-psychological) factors can have a role in determining its content. This is actually the route taken by Feyerabend and Kuhn, especially in connection with radical theory changes. It is also the view of Bloor (some qualifications of his, notwithstanding).⁴⁶

The arguments that have generally been employed against an externalist account of the content of scientific theories can be divided into three important types - the empiricist argument, the rationalist argument and the self-refuting argument. I shall examine each of them in turn. We shall see that none are tenable because they each presuppose either an inadequate view of scientific experience or scientific rationality.⁴⁷

(a) The Empiricist Argument

It has been argued that scientific theories are constrained by the empirical data in such a way that it is impossible for them to be affected by sociocultural factors. This argument has been taken as sufficient to preclude any possibility of a sociological theory providing an account of the content of rationally accepted scientific theories. Even some of the founding fathers of the sociology of knowledge, like Merton and Mannheim,⁴⁸ have adopted this position. A more recent argument of the same kind has been given by Richter who declares "society cannot, in principle, determine the contents of scientific knowledge, because these are to be determined by observations of nature."⁴⁹

This empiricist argument is valid only if we presume certain types of theories of scientific rationality. Newtonian inductivism provides a classic model of a theory of knowledge that automatically precludes any possibility of an external account of the history of science. According to the Newtonian model the data are given directly to observation, and providing care is taken not to go beyond what is given to the senses, such observation reports provide an incorrigible foundational basis for knowledge. Furthermore, theories could be induced from these phenomena without the intrusion of any conjectural element. Thus, both the observation reports and the theory of science are dictated in toto by the events in the world. Such a view espouses, what we may call, the empirical determinism of theories. On this account, it would be impossible to suppose that the content of a scientific theory could be affected by sociocultural factors.

What is the scope allowed for the sociology of knowledge by such an epistemological theory? Clearly, it allows that the sociologist could attempt to explain why scientists select particular domains or problems for study; it could also allow studies of the institutional factors that affect the growth of scientific knowledge. But, insofar as accounting for the content of scientific theories, it allows no scope for any sociological theory. The only time a sociological account can be provided for explaining the content of theories is when scientists, departing from scientific grace, allow their prejudices and biases to influence their observation reports or theories; when they carelessly go beyond what is allowed by the data in arriving at theories; or when they allow their interests to affect their judgement. Thus, the only kind of cognitive sociology of knowledge allowed by the Newtonian inductivist model is what Bloor has disparagingly referred to as a 'sociology of errors'.⁵⁰

Let us now examine an alternative model of science - that of hypothetico-deductivism - to see how much scope it allows for a cognitive sociology of knowledge. Let us assume that the model supposes that facts are directly given to observation, but that theories are merely conjectures that have to be tested by examining their ability to accommodate such given facts. Clearly, such a model allows for sociocultural factors to affect the content of theories, but their influence is extremely limited. For, there is an infinity of facts a theory can predict, and an unlimited number of facts that can be observed. By widening the base of facts indefinitely one can attempt to minimize to a great extent the effect of sociocultural factors in determining the content of scientific theories. Sociocultural

influences may be allowed in the context of actually creating theories but, once a theory has been created, it can be tested rigorously by appealing to an indefinitely large and autonomous domain of facts which are not socioculturally determined. Thus, the sociocultural influence that exists at the level of discovery of theories can be systematically minimized (if not totally eliminated) in the context of justification. Again, the only scope allowed the sociology of knowledge is either a sociology of errors or a noncognitive sociology of knowledge.

It is only by recognizing that there is no theory-free (and hence socioculturally independent) domain of facts that we can allow a thoroughgoing cognitive sociology of knowledge. I.e., it is the expectational nature of observation reports - their theory-ladenness - that provides the foundation upon which a cognitive sociology of knowledge can be erected. This is because there is now no socioculturally independent domain that can be appealed to, either in the testing of theories or in the observation of events in the world. The empiricist argument against externalism - that theories are totally constrained by the data - collapses when the data themselves are constituted in part by our theoretical knowledge.

Such a view, however, does not allow us to adopt a radically externalist account of scientific knowledge that would deny any role for a theory of internal rationality. The theory-ladenness of experience does not mean there are no constraints on the data. The world of experience is constituted by gestalts, but even if these are theoretically structured they are also organized around given sense data. It is the radical nonfoundationalism of holistic empiricists, like Kuhn and Feyerabend, that leads them to deny that there can be any

specific theory of internal rationality to evaluate scientific theories. We have seen that such a theory is possible. However, the theory we have proposed is not one that allows us to say that the external data constrain theories to such an extent that an evaluation of them as rationally acceptable furnishes a rationally acceptable explanation of their content.

This double determination of the content of theories by the world of experience and the world of culture is recognized by Bloor, but he is unable to effect their reconciliation. Instead, he opts for the view, as we have seen, that they are 'two opposed metaphysical standpoints' that cannot be effectively synthesized or effectively rejected. Thus he writes:-

"... what we count as scientific knowledge is largely theoretical. It is largely a theoretical vision of the world that, at any given time, scientists may be said to know. It is largely to their theories that scientists must repair when asked what they can tell us about the world. But theories and theoretical knowledge are not things which are given in our experience. They are what give meaning to experience by offering a story of what underlies, connects and accounts for it. This does not mean that theory is unresponsive to experience. It is, but it is not given along with the experience it explains, nor is it uniquely supported by it. Another agency apart from the physical world is required to guide and support this component of knowledge. The theoretical component of knowledge is a social component, and it is a necessary part of truth, not a sign of error."⁵¹

The reason Bloor has to view internalism and externalism as irreconcilable is now apparent. Though he recognizes the underdetermination of theory by experience, he cannot defend it consistently unless he also allows for the theory-ladenness of experience. For, if experience is theory-independent, then it is possible to widen the base of experience against which a theory can be tested so as to diminish the influence of cultural factors. However, if

he allows for radical theory-ladenness, he could undercut his materialist empiricism totally by collapsing into the camp of the holistic empiricists (who, with their radically incommensurabilist view of different world-conceptions, seem to deny that theories can be constrained by the world at all).

(b) The Rationality Argument

Laudan has argued the case for a demarcation between the sociology of knowledge and an internalist account of science in the following manner. Following Mannheim he distinguishes between immanent ideas and non-immanent (existentially determined) ideas. Immanent ideas are those 'which can be shown to be naturally and rationally linked to other ideas to which a believer adheres'.⁵² He refers to the theorems of Euclid's geometry as an archetypal example of immanent ideas. For once the axioms are accepted, one is rationally constrained to accept the theorems. On the other hand 'non-immanent ideas are those which do not carry their rational credentials with them'.⁵³ Laudan then argues that it is only non-immanent ideas that it is appropriate for a sociology to attempt to explain for such ideas are 'not rationally the most well-founded in a given situation'. This allows him to formulate his criterion of demarcation - the arationality assumption:-

The sociology of knowledge may step in to explain beliefs if and only if those beliefs cannot be explained in terms of their rational merits.⁵⁴

Laudan establishes two separate domains of inquiry - one for the historian of ideas and the other for the sociologist of knowledge. This 'division of labour' allows the historian of thought to perform the task

of explaining science, insofar as it is 'rationally well-founded', and leaves the sociologist of knowledge the role of performing the mopping up operation of accounting for those ideas that did not conform to what would have been allowed by a rational analysis of the situation. This is nothing more than a demarcation criterion between the history of ideas and a sociology of errors. A similar criterion has been presented by Lakatos.⁵⁵

The only example that Laudan furnishes for what it means for ideas to be immanently connected involves logical deduction - the theorems of geometry logically follow from the axioms that have been accepted and, hence, axioms and theorems are immanently connected. However, if we are to take logical deducibility from accepted ideas to be the criterion of immanence, we would have to conclude that the sociology of knowledge is required to explain practically all of science (except logical deduction). For any scientific theory, even Newton's theory of gravitation, say, is not a deductive system like geometry. In every concrete application it requires supplementation by all sorts of auxiliary theories. Such auxiliary assumptions are often not given in advance, but created in the context of the situation. To explain, say, the motion of the moon, we are required to make all sorts of assumptions about the influence of other planetary bodies, its internal structure (e.g., is it of uniform or non-uniform density?) and so on. Furthermore, the initial conditions are determined by observation reports that are supported by our theories of optics and the telescope, or theories about the effect of the atmosphere on what we observe. Thus, if Laudan intends to separate internalism and externalism by defining immanently connected ideas as being logically connected, he would hand over to sociology of

knowledge all of scientific knowledge. Though this is precisely what should be done, it is not Laudan's position.

Clearly, Laudan's example is misleading because he is far from being a logicist. It is possible to view immanent connection in terms of a theory of internal rationality - ideas are immanently connected if they are linked in such a way as to make them acceptable when seen in the context of internal rationality. They are then part of a system of theoretical assumptions that can be evaluated as rationally acceptable. However, such a connection does not require us to accept the arationality assumption. For even if the content of a theory - i.e., the ideas contained in the theory - can be evaluated as rationally acceptable, a sociology of knowledge could still step in to explain the content of these ideas. Evaluating a system of ideas as rationally acceptable by appealing to a theory of internal rationality - such as in terms of the relative explanatory indispensability - does not preclude explaining them by appealing to cultural factors.

Even Bloor falls for the bait thrown out by this rationalist argument. He concedes too much when he allows that, if a theory of internal rationality can account for why a theory is acceptable, then the content of the theory has been explained. This is what leads him to suppose that the teleological model and the causal model (internal and external models) are 'two opposed metaphysical standpoints'. However, if we adopt the standpoint that to explain the content of a theory is to do more than evaluate its content as acceptable, the scope of the sociology of knowledge becomes all of knowledge. Furthermore, this extension in scope is not bought at the price of denying the possibility of an internalist account of science - one that seeks to delineate the

evolution of increasingly adequate explanatory theories.⁵⁶

(c) The Self-Refuting Argument

This argument has been presented as one that purports to show that any thoroughgoing sociology of knowledge is bound to end in a reductio ad absurdum, or in total relativism. There are many writers in the literature who have used it to refute the possibility of a cognitive sociology of knowledge.⁵⁷ It has recently been reformulated by Laudan:-

There are theoretical as well as practical reasons for laying down in advance some way of deciding on the boundaries of the potential problems for the cognitive sociologist. If it were true that all beliefs were not the result of rational deliberation or enlightened evaluation, but rather were simply determined by the social situation of the believer, then the whole enterprise of cognitive sociology would be self-indicting; for if all beliefs are socially caused, rather than rationally well-founded, then the beliefs of the cognitive sociologist himself have no relevant rational credentials and hence no special claims to acceptability.⁵⁸

Let us examine the validity of this argument more closely. In the first place, it assumes as Bloor points out, that 'social causation implies error'. It assumes that beliefs that have been socially caused can have no relevant rational credentials. There is no reason to accept this premiss.⁵⁹ For though a belief may have been socially determined, there may also have been rational grounds for holding it. E.g., the belief that the sun is at the centre of the universe may be causally connected to social circumstances and, yet, there may also be rational grounds for holding it. Similarly, the belief in an acausal theory of physics, or a mechanical theory of evolution, may be affected to a great extent by sociocultural causes,⁶⁰ but this does not mean that there cannot be rational grounds for holding these beliefs.

It may be said that if a belief is socially determined then rational factors could have played no role in the acceptance of such a belief. This would be true only if the theory were totally determined by the social situation. No sociologist of knowledge need make such a claim. For the theory in question, even if it arises in a social context, also addresses itself to a reality that lies external to society. For a sociologist to claim that a scientific theory is totally determined by social factors is to refuse to recognize that the theory is also required to explain events that are not social at all. To assert that the content of a theory is socially determined is not to claim that the content is only determined by the social situation. It is only to claim that the social situation affects the content of the theory. Thus, the content of the theory could have been determined both by external social factors, as well as the requirement of acceptably explaining a domain of events.

There is no incompatibility in showing that a theory is causally determined, and in showing that the same theory is also rationally well founded. Using the analogy raised earlier there is no incompatibility in showing that the design of a racing car is determined by social factors as well as affected by the requirement that it be a good racing car. The performance of the car can be explained by appealing to its design without invoking sociocultural factors. But to explain its design we not only need to know that it is constructed to be a good racing car, but also that it is constructed in a particular sociocultural environment.

Consider the core-objection embodied in the self-refuting argument. It is said that the sociological theory is also socioculturally

determined. But why is this to be construed as a definitive refutation of the theory? It will only be a refutation of any sociological theory that denies that there can also be a theory of internal rationality. Just as the content of a scientific theory can be internally evaluated as rationally acceptable, but nevertheless be socioculturally determined, so can the content of the sociological theory be internally evaluated as well as explained by appeal to external factors. The sociological theory is not required to explain its own content - only that of the theory it addresses itself to. (This is, of course, not to preclude the sociological theory being applied to itself self-reflexively in another context. We shall see later that self-reflexivity is not a trouble objection).

The situation may be clarified by using an analogy. Take the game of chess. One can analyze the game in terms of the moves made by each player and show how, given the situation in each case, both players made rationally acceptable moves. I.e., one can account for the moves by giving reasons that appeal to the internal rationality of the game. Such an account has its merits in a certain context, but it cannot be said to give a total explanation of the moves in the game. To do this we have to take into account the styles of the two players, and their background experience (including their cultural context). Russians have a style quite distinct from Americans. Though one can analyze a game between players from these two countries in terms of their internal rationality, one cannot suppose that one can explain completely the nature of their moves by means of such an analysis. An internalist account can tell us why the players made acceptable moves, but it cannot furnish a complete explanation of them. To give a total explanation of

these moves we have to take the fact that the players were constrained to make rationally acceptable moves as a datum, but not as the only datum.

Any theory of cognitive sociology, however, must satisfy two reflexivity conditions if it is to be considered an acceptable scientific theory. They may be formulated as follows:

1. The pattern of explanation for the content of scientific theories it offers must also be applicable to the sociological theory itself.
2. The sociological theory must be evaluated by the same sort of procedures as are applied to evaluate other scientific theories.

This is to say that if a sociological theory is used to explain the content of other scientific theories, it must also be capable of being used to explain its own content. Thus, the rational explanation it offers of the content of other theories must be applicable to itself. Secondly, the sociological theory must be evaluated by the same theory of internal rationality that is employed to evaluate other scientific theories. Any accepted sociological theory has to be internally evaluated as acceptable. Let us call these the externalist and internalist reflexivity conditions respectively.

The externalist reflexivity condition has been proposed by Bloor but, since he presupposes along with internalists like Laudan that a theory of internal rationality is incompatible with a thoroughgoing sociology of knowledge, he does not require the internalist reflexivity condition. However, sociological theories are used only to explain the content of scientific theories, not to evaluate this content.

Nevertheless, such sociological theories have to be evaluated with respect to their ability to explain the content of the theories they are addressing. Internal reflexivity requires that the way we compare

theories of cognitive sociology cannot be different from the way we judge other scientific theories.

The domain of theories of cognitive sociology is the content of other scientific theories. The sociological theories are required to explain the content of these theories by appealing to sociocultural parameters. It is true that the content of an explanatory sociological theory is subject to sociocultural influences. There is no reason not to suppose that the content of the sociological theory is, itself, explained by another sociological theory. This does not indicate that the sociological theory cannot furnish an acceptable explanation. Just as other scientific theories can provide a rationally acceptable explanation of the events in their domain, even if their content is socioculturally influenced, so can a sociological theory offer a rationally acceptable explanation of the content of other theories (even if its own content is socioculturally determined).

It may be argued that I have ignored an important difference between sociological theories of knowledge and other theories. The sociological theories explain the content of other theories in terms of sociocultural causes that operate on the sociological theories themselves. Other scientific theories do not explain the events in their domain in terms of causes that affect the content of these theories. Hence, they do not involve themselves in a vicious circularity.

This argument is misleading. Materialistic theories account for the world in terms of material causes. The same material causes must therefore account for material theories, because such theories are events in the world. If there is any circularity it must also operate here. Similarly, theories of human behaviour must include theorizing as

a part of human behaviour; and neurophysiological theories must include the neurophysiological processes involved in constructing them as causes that influence the content of these theories. In fact any theory, that applies to the world as a whole, must include the person who theorizes, the activity of theorizing, and the theories that arise from this activity, as something to be explained by the theory. This does not indicate that there is any circularity involved in such theories. Thus, there is no circularity in supposing that a sociological theory explains in terms of causes that affect the content of the theory itself. Nor is there any circularity in supposing that the theory can be used to explain its own content.

Clearly, the circularity would arise for any sociological theory that denies the possibility of a theory of internal rationality to evaluate scientific theories. This is the case, for example, with Bloor's strong program for the sociology of knowledge. For then, even though the theory may satisfy the external reflexivity condition, it cannot acknowledge an internal reflexivity condition. Thus, competing sociological theories that seek to account for the content of the same scientific theories cannot be rationally evaluated. However, given a theory that can satisfy both reflexivity conditions, there is no reason to deny the possibility of a thoroughgoing sociology of knowledge. The nature of the specific content of such a theory - the factors in terms of which it would explain the content of other theories - is not one that an epistemological treatise such as this is required to investigate.

5.6 The Perspectives of Explanation and Evaluation of Theory Content

The above discussion reveals the necessity of distinguishing between two different ways of approaching the content of scientific theories. In one of them we are concerned with the ability of this content to account for the domain of phenomena the theory seeks to explain. In the other we are concerned with explaining the content of the theory. The first requires that we have a theory of internal rationality to evaluate the content of the theory in question. The second calls for a sociological theory to understand why the content of the theory is what it is. The same content may be treated from two different perspectives. We may adopt an evaluative perspective - and thus confine ourselves to an internalist judgment of the theory vis-a-vis other theories - or we may adopt a perspective of explaining the theory-content - and thus be involved in treating the theory-content from a sociological dimension.

The distinction between the perspectives of providing a rational explanation and a rational evaluation of theory-content may remind one of a similar distinction made by the logical empiricists - the distinction between the context of discovery and the context of justification of theories.⁶² There are reasons to suppose that this distinction is an untenable one. Theories are not static entities that are discovered once and for all. They evolve, and their evolution is affected simultaneously by sociocultural influences and constrained by their need to explain the events in their domain. The process of justification of theories occurs along with the process of discovering theories. The context of discovery, itself, involves the context of justification. Furthermore, sociocultural influences are pervasive

throughout this process. Thus, it appears more fruitful to distinguish between two different perspectives we can adopt with regard to the content of theories than to suppose that theories can be understood by differentiating two temporally separated contexts. Throughout the evolution of a theory we may adopt the perspective of evaluation if we are concerned with the theory as an explanatory system. Alternatively, if we are more interested in explaining the content of the theory we may adopt the perspective of explanation to deal with its evolution.

The perspective of explanation of the theory-content has to include the perspective of evaluation. For the theory-content, even if it is affected by sociocultural factors, is also determined by the explanatory role required of the theory. Thus the two perspectives are not mutually exclusive. The perspective of evaluation is a limited perspective - one furnished by a theory of internal rationality. The perspective of explanation is more inclusive. If we require a theory of cognitive sociology for the perspective of explanation, this theory must include as part of its data the theory of internal rationality used to evaluate theories.

The distinction between the perspectives of rational explanation and rational evaluation of theory content is quite different from the distinction between the context of discovery and the context of justification of theories. For one thing the contexts of discovery and justification are temporally separated. Secondly they are considered to be mutually exclusive. This is not the case with the two perspectives. These can be adopted simultaneously with respect to the content of a theory, and one of them (the perspective of explanation) includes the other.

Separating the perspectives of explanation and evaluation enables us to separate the internalist and externalist approaches to science. Within the internalist perspective of content evaluation we are not required to explain the content of theories. We are only required to evaluate the adequacy of this content to explain events. Within the externalist perspective we have to explain the content of a theory. It is not surprising that, in explaining the content of a theory, we also have to take into account the ability of the content to explain a domain of events. Thus an externalist account of the content of a theory is more global, and includes the internal dimension; but the more limited internal account can ignore the externalist perspective. A program for the Sociology of knowledge would adopt the perspective of explanation of theory-content, and a program of internalist rationality would adopt the perspective of evaluation of theory-content.

5.7 Paradigms and the Sociology of Knowledge

It is at the level of paradigms, rather than theories, that sociocultural factors play the most important role in determining the content of scientific theories. This does not, of course, mean that social influences are not involved in affecting science when we make observations or formulate theories within a paradigm. However, at the level of individual observations or paradigm directed theorizing the effects of sociocultural forces are far more constrained. This is because theories developed within paradigm are, to a large extent, immune to such external influences, since they are constrained by the network of metaexpectations offered by paradigm.

For example, it can be argued that Darwin's theory of evolution by natural selection had a paradigmatic core much influenced by the doctrines of economic individualism prevalent in the 19th century. It is more difficult to see how neo-Darwinian theories today, that are already constrained by the Darwinian paradigm, can be affected to the same extent by presently prevalent sociocultural influences. Similarly, Copernicus' paradigm and the quantum theory were both influenced to a great extent by the cultural matrix in which they emerged. However, once these paradigms became accepted their further evolution was far less sensitive to external factors, and far more dictated by internal factors, than at their inception. For in accepting the original paradigm one has already accepted a great deal of constraint in the development of one's theories. Even if there is a sociocultural influence, this is restrained by the internal requirements on a theory imposed by the accepted paradigm. Thus, once a paradigm has been formulated, we may expect the development of theories within that paradigm to be relatively more autonomous from culture. This does not mean that it can be assumed to be completely free from the influence of culture and interests.

The fact that development within a paradigm can be relatively autonomous does not mean that sociocultural influences have been eliminated. This is obvious, because the paradigm itself has been shaped by social forces and embodies their influence even if it has attained relative autonomy. E.g., public institutions may become relatively autonomous from external social influences, but this does not mean that the institutions were not born out of social interests, or have ceased to embody such social interests. Similarly, though

paradigm-determined theorizing may appear socially autonomous, it nevertheless embodies social influences. Furthermore, since the network of metaexpectations offered by the paradigm guides every form of theorizing within its framework, every theory articulated out of a paradigm is permeated by the sociocultural influences that determined the content of that paradigm in the first place.

Now, the metaexpectations of the paradigm are the deepest and most pervasive expectations in any one of its instantiating theories. The theory is only a more specific articulation of the general expectations given by the paradigm. Thus the effects of social influences upon a theory are at the level of the most pervasive expectations contained in it. They are constraints imposed upon all other more specific expectations that are added to articulate the theory out of the paradigm. This means that the sociocultural influence upon a theory cannot be considered to be a minor matter. This influence percolates through every aspect of the theory.

The fact that a theory's content is permeated through and through down to its deepest level by sociocultural influences does not mean that a theory cannot be true. It can be said to be untrue only if we presuppose a theory of truth that requires that theories can only be true if their content is free from any sociocultural influences. On the other hand, if we allow that a theory can be true if it provides an (idealized) acceptable explanation⁶³, then a theory with a socioculturally determined content can be true.

The reason why sociocultural factors are most influential at the level of paradigms is due to the very generality of the metaexpectations embodied in them. Being so general, these metaexpectations are the

least constrained by the events in the world. The only constraint upon them is that they lead to instantiating theories that are capable of representing the events in their domain. This is, of course, an empirical constraint that is not sociocultural, but within this limit there is a wide scope for adjustment. A paradigm is not easily testable against experience. We can modify its instantiating theories, create new auxiliary theories, or change observational theories so as to align empirical evidence to the paradigm. Though this cannot be done with arbitrary freedom, it is one of the major reasons why a paradigm will not be rejected except without the emergence of an alternative whose instantiating theories eliminate the theories of the paradigm. Thus, the metaexpectations of paradigms, being so difficult to test and being only remotely connected to the world of events, are the most susceptible to influences and interests of social origin.

The view that paradigms are systems of metaexpectations enables us to integrate a program for the sociology of knowledge with the empiricist dimension of the scientific enterprise. In the traditional view the ontological presuppositions embodied in paradigms were viewed as descriptive claims about the world. Clearly, if they were descriptive, and sociocultural influences affected such descriptions, then these claims would have to be seen as distorted by such influences. For the more the paradigm is purged from external influences the purer would be its descriptive component. The argument is straightforward, for there can be no reason to suppose that reality would conform to the demands we impose upon it on the basis of our cultural interests.

On the other hand, if ontological presuppositions are expectations - i.e., both demands on how reality should be represented as well as

claims about how reality can be represented - then it is possible to allow that cultural factors may affect the way we are required to represent reality. Furthermore, unlike what we are lead to suppose on the descriptive view of statements, we cannot take such a cultural influence to be a distorting factor. For the aspect embodying demands is an integral part of statements in the expectational view; only in the descriptive view would it appear as a distorting effect. The only constraint upon these culturally instituted demands is that they lead to instantiating theories that are acceptable representations of the world.

Similarly, methodological principles in paradigms that specify the content of theories would have been considered improper in the traditional view. For this view held that since reality is described by our theories, we cannot prescribe the content of theories: this would be to prescribe the nature of reality. However, if methodological principles are seen to be expectations then, such expectations being simultaneously demands on how reality should be represented and claims about how it can be represented, they can be susceptible to both cultural and empirical influences at the same time.

Thus, the fact that cultural influences, needs and interests affect our metaexpectations in no way diminishes the objectivity of science. It only requires us to reject that view of scientific objectivity based upon a descriptive view of statements, and which requires that the representations of reality we construct must be totally divorced from any demands imposed by us. If we adopt the expectational view of statements, then the statements that represent reality are constituted both by demands as to how reality should be represented and claims about how it can be represented. The demand aspect may be affected by

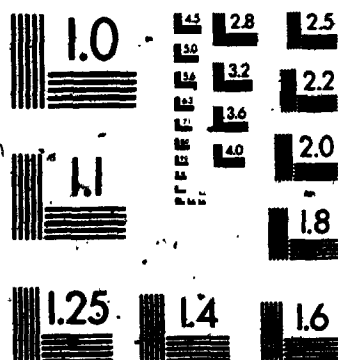
cultural factors and the claim aspect by empirical factors, but the two cannot be separated. The metaexpectations embodied in paradigms constitute the meeting ground between the world of external events and the human world of culture and interests. They prove their objectivity by their capacity to represent the culturally independent domain of events; and they show their cultural component in their sensitivity to social influence and human interest.

REPRESENTATIONAL TRUTH AND PERSPECTIVAL REALISM6.1 Introduction

Traditionally the goal of science has been held to be the pursuit of truth. Contemporary philosophers have found this notion so problematic that they have often sought to exclude the concept of truth from their epistemologies altogether, or even deny that truth can be a goal that scientists could be said to strive after.¹ However, if there is a problem with the notion of truth what reason is there for supposing that this notion is irrelevant? May it not be that the problem is not with truth, but with the theories we hold about truth?

The three classical theories of truth - the correspondence, the coherence and the pragmatic theories - seem inadequate to deal with the problem of truth in the sciences. Each of them offers a characterization of what it means for a statement to be true, but none of them appears to capture the notion adequately. The correspondence theory conceives that a statement is true by virtue of a connection that exists between it and the world. But a statement can also be held true by virtue of the connection it has with other statements we have accepted. This has lead some philosophers to emphasize a coherence theory of truth. Furthermore, as we saw in the last chapter, theories often embody human interests and cultural values. Hence the statements we accept often embody such values and interests. Pragmatic theories emphasize this utilitarian dimension of statements but, since utility is often a variable commodity depending on whose utility is involved,

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these theories are often hard-pressed to explain how they can provide an intersubjectively acceptable characterization of truth when those interests are in conflict.

What is required is a theory that takes into account the complementary aspects of statements and meanings inherent in the expectational view and the holarchic theory. It must also recognize that the content of a true statement could embody sociocultural interests. The correspondence theory fails on all counts; the coherence theory, instead of recognizing the complementary aspects of statements and meaning, makes truth excessively dependent on language alone. Though the pragmatic theory is more capable of accommodating these requirements it, nevertheless, is inadequate because of many of the traditional objections that have been raised against it. In the place of these theories, I propose a representation theory of truth. This theory is not a variant of the correspondence theory - as is normally assumed. It is actually a nonpragmatic alternative to the correspondence and coherence theories that, not only allows us to recognize that truth has a double dependence on language and the world, but also captures many of the crucial insights of the pragmatic theory without the latter's inherent deficiencies.

The representation theory leads us to recognize that there can be a plurality of true theories of the world. This suggests that we cannot interpret the entities postulated by true theories in realist terms. Neither is an instrumentalist or constructivist account of them satisfactory. We shall see that along with other epistemic notions - statements, meaning and truth - the objects that are postulated by true theories also exhibit complementary aspects. They, too, have a double

dependence on language and the world. For this reason I have called them perspectival objects. Such objects have to be construed as both invented and found (or created and discovered) by the scientific investigator. It is only by relinquishing the view that these objects are either only real or only constructed that we can save the most basic presumption of scientific realism - that there is an external world independent of our minds and, at the same time, allow that there could be a plurality of true theories of the world. Let us begin with a critical examination of the classical theories of truth.

6.2 Critique of the Traditional Theories of Truth

The oldest of these is the correspondence theory of truth. It asserts that a sentence can be said to be true provided it corresponds to a fact. Thus one may say that the assertion "The sun rises in the east" is true if, and only if, it is a fact that the sun rises in the east. Such a theory accords well with the commonsense notion that the truth of a statement depends on some sort of relation between the statement and a world independent of our propositions. The relation in this case happens to be that of correspondence. Such a theory bears a very close resemblance to a picture theory of truth - e.g., that offered by Wittgenstein - where true statements are considered to somehow reflect whatever is the case in the world.²

There are, however, numerous difficulties that arise if we subscribe to a correspondence theory of truth. In the first place we have to decide if the theory provides us merely with the meaning of truth, or does it also offer a criterion of truth. There are some philosophers of very different persuasion who allow that the theory does

offer the meaning of truth - e.g., Popper and Rescher - but, whereas Popper denies that we need a criterion of truth, Rescher argues that the criterion ~~must~~ be coherence.³ Secondly, the notion of facts is one that is problematical. It may even be argued that there are no such entities as facts at all.

The area of greatest success of the correspondence theory appears to be in its ability to deal with observation reports.⁴ Once we have been given the meanings of the terms employed, it seems natural to assume that when we make a report like "Water boils at 100°C at one atmospheric pressure", or "Copper expands when heated", our statements are a direct consequence of observation, and bear a correspondence to what has been observed to be the case. I.e., our statements correspond to the facts in the world. In the coherence theory the truth of these statements cannot be established so directly - it also depends on the other statements of our system; and within a pragmatic framework we may not find it useful (whatever criteria of utility we employ) to believe in these propositions. Thus, in the case of both these theories, observation reports have no special status compared to other statements; or we have to invoke criteria outside the theory to give observation reports a special status. The correspondence theory does not require us to do this. Since, the observation reports are the result of a direct confrontation with the facts, their truth is founded upon their correspondence to these facts. The facts are what is observed and the statement, in expressing these facts, indicates its correspondence to them. Thereby, it shows itself to be a true statement.

If we go along with this view we can see that the correspondence theory furnishes us with both the meaning and the criterion of truth for

observation reports. The meaning of truth is correspondence to the facts. The facts are directly accessible to observation. The statement - once its meaning is given - can be seen to correspond or not correspond to the facts. If it does then it is a true statement. This is what it means for a statement to be true. At the same time this procedure also furnishes us with the criterion of truth. For the truth of a statement is decided by examining if it corresponds to the facts. If the answer is in the affirmative then the statement is true. Thus, correspondence gives the meaning as well as the criterion for the truth of a statement.

The disarming clarity of the correspondence theory, however, dissolves once we examine the notion of facts more closely. This is because facts are not the sort of things that can be deemed to be given unambiguously.⁵ Take the observation report "Copper expands when heated". What does it correspond to? It certainly cannot be said that the correspondence is to a particular event in the world - namely that a particular bar of copper expanded when heated. It is a more general assertion than that. Neither can it be said to be about a class of events that have been observed, for it also asserts that copper will be observed to expand in the future. Thus the statement is about events that have been observed in the past, and also about events that are likely to occur in the future. In this case what is the fact that the statement corresponds to?

The difficulty here may be said to be due to the statement itself. I.e., it may be claimed that the statement "Copper expands when heated" is not really an observation report. It goes beyond observation because all that observation can tell us is that "This particular bar of copper

in this particular location expanded when heated at this particular time". However, such a formulation of an observation report is hardly relevant for science, for it cannot provide any support for scientific theories. Scientific theories can appeal to reports of the form "Copper expands when heated" for support, but the statement that a particular piece of copper at a particular location in space-time expanded, when heated, is hardly the sort of observation that is general enough to provide any such support.⁶ Furthermore, the statement itself presupposes that what was heated was copper, and that it expanded because it was heated. But if the theory of relativity is correct it may have been observed to expand because we have changed our frame of reference from a moving to a stationary one (with respect to the bar of copper). Only if we suppose that we can identify copper, and that we have not changed our frame of reference - both of which are suppositions that can only be supported by other statements - can we claim the result that we have observed, i.e., that there was a causal connection between the heating of the copper bar and its expansion. Thus, the so-called fact that the statement reports is not as directly given to us as we might imagine.

This means, however, that the fact which we identify cannot be dependent only on the event that gives rise to it. This is, of course, something that correspondence theorists would be prepared to allow. Indeed they do claim that facts and events are to be distinguished. For though facts are nonlinguistic elements they are not events, because a distinction can be drawn between an event and the fact that such an event occurred. There may be facts about events, but there are no events about facts.

Furthermore, facts are not statements - even true statements. For if we identify facts with statements then the correspondence theory breaks down. To say that a statement corresponds to the facts would be to say that it merely corresponds to another statement. Another reason why facts cannot be identified with statements is that facts are said to exist whether or not we know them. E.g., a correspondence theorist would want to say that the statement "There is life outside the solar system" may correspond to a fact even though we do not know if it does.

However, in adopting this position we seem to land ourselves in a very peculiar situation. The correspondence theory requires us to suppose not only linguistic statements and events in the world, but also a whole new ontological dimension - a so-called world of facts. We observe the events in the world; we recognize the facts on the basis of this observation; and we formulate true statements that correspond to these facts. Thus, insofar as correspondence is the relation we require to establish truth, we seem to force ourselves into inventing a third ontological world to sustain this relation.⁷

Furthermore, there is a certain peculiarity about this world of facts. For the issue of whether something is a fact appears to depend a great deal on the sort of theories that we have. E.g., that the moon is a planet was considered a fact when the Ptolemaic theory was held to be true, but it ceased to be a fact once the Copernican theory was generally adopted. If facts can undergo this sort of mutation in a theory-dependent way, what sense does it make to say that facts exist regardless of whether we are aware of them? It would appear that the entities that are in this 'third world' are intimately constituted by our theories about the world of events.

Of course, a correspondence theorist could rebut the above objection. He could assert that it was not a fact that the moon was a planet even when the Ptolemaic theory was held to be true - it was only considered to be a fact, but this view was mistaken. However, this means that we can be mistaken as to the identification of facts. In that case we may be mistaken if we claim that a statement is true because it corresponds to the facts, since what we consider to be the facts may not be facts at all. Unless we are given some means of identifying facts unambiguously, how are we to decide if statements are true?

Some philosophers have sought to avoid this problem by distinguishing between the meaning of truth and the criterion of truth. They have held that the correspondence theory does not provide us with a criterion that enables us to decide whether a statement is true. Popper, e.g., holds that there cannot be such criteria; Rescher argues that, though correspondence furnishes the meaning of truth, the criterion of truth is coherence. But this, however, makes the correspondence theory completely vacuous. We are told what it means for a statement to be true, viz., it corresponds to the facts. However, what these facts are cannot be identified in any unambiguous way - or even in any way. However, whatever they are, we know that a statement can be said to be true only if it corresponds to them. In practice, however, we can only approach the truth by means of criteria that are not provided by the correspondence theory.

It may be argued that the meaning of truth offered by the correspondence theory provides us with a regulative principle. This appears to be the position adopted by Popper. The correspondence

theory is regulative in the sense that, though it furnishes us with no criteria that enable us to decide if a statement corresponds to the facts, it guides us in the selection of criteria that are more likely to lead us towards the truth. Thus, a correspondence theory of truth would require us to suppose that truth would have to satisfy the criteria of coherence, as well as pragmatic criteria. For if a statement is true according to the correspondence theory it would have to be coherent with other true statements; also a theory that corresponds to the facts will be more useful than one which does not.

.. should there exist something like the correspondence of a theory to the facts, then this would obviously be more important than mere self-consistency, and certainly also much more important than coherence with any earlier 'knowledge' (or 'belief'); for if a theory corresponds to the facts but does not cohere with some earlier knowledge, then this earlier knowledge should be discarded.

Similarly if there exists something like the correspondence of a theory to the facts, then it is clear that a theory which corresponds to the facts will be as a rule very useful; more useful qua theory, than a theory which does not correspond to the facts.

This argument is misleading. For a theory may increase in coherence without thereby approaching anything like the correspondence to the facts. Thus, while correspondence may require coherence, coherence in practice may not lead us towards correspondence. The same may be said of pragmatic theories. Thus the correspondence theory cannot even function as a regulative principle if, in practice, this regulative principle can only allow us to judge truth in terms of coherence or pragmatism. This being the case, one can ask why it is necessary to adopt a correspondence theory at all?

Most of the difficulties that are associated with the correspondence theory can be traced to our inability to isolate facts

independently of our theories; our inability to decide if a statement is true without considering its relation to other statements; and our inability to show that the kind of statements we make about the world are independent of sociocultural influences and interests. If all statements (including observation reports) are theory-laden, we cannot identify facts independently of our theoretical system. Thus the notion of correspondence to facts becomes vacuous. Furthermore, if the truth of a statement can only be judged in relation to other statements, its truth cannot be established merely by indicating that it corresponds to an extra-linguistic element like a fact. Even worse, if our statements are a part of conceptual paradigms that are intimately constituted by sociocultural interests, then they cannot merely correspond to facts that exist independently of these interests.

The difficulty in isolating facts arises, for example, when we take assertions about the past. Is a statement like "Dinosaurs roamed the earth 50 million years ago" true? If we claim it to be true then we must, according to the correspondence theory, acknowledge that it corresponds to a fact. However, what is the fact that it corresponds to? Even if dinosaurs did roam the earth 50 million years ago that event cannot be the fact that the statement corresponds to now. For the correspondence theory to be held acceptable we have to suppose that the statement corresponds to a fact that exists now. The only way out is to suppose an ontological dimension of immutable facts, but this has its problems. The same sort of difficulty arises with respect to the future. A statement like "The sun will rise in the east tomorrow" may be said to be a true statement, but what does it correspond to? This event has not yet occurred. Or are we to suppose that a fact pre-exists

the occurrence of the event about which it is a fact?

The basic problem in each of these cases is that the statements we make are not about present events. They are based upon a theoretical framework that allows us to make statements about events that have already happened, or are expected to happen in the future. Thus, we base our claim about dinosaurs on the observations of bones and other evidence that exists now, but our statements are not about current events. A chain of theoretical reasoning connects the events we have available now to events in the past. In the same way a chain of theoretical reasoning connects the events now given to us to an event in the future. Our statements about events in the past seek to present these events in our conceptual system, but the representation itself depends on a complex network of theories. The statements are true if they adequately represent these events, but there is no reason to suppose that they are true because they correspond to presently existing facts about these events.

The same problem also arises for the theory in connection with statements that are universal in scope. Take the statement "Every particle in the universe possesses mass". How can we establish the truth of this statement by showing that it corresponds to a fact? To do this we would have to either appeal to some sort of intuition into a world-of-facts, or we would have to show that this is the case with every particle in the universe. It is difficult to see how either approach is credible in practice.

The difficulties confronted by the correspondence theory in dealing with general statements, and those about past and future events, resides in the theoretical nature of these statements. This also creates

problems for the theory in dealing with probabilistic and hypothetical statements. Such statements are an important feature of scientific theorizing. The statement "The probability that this particular radioactive atom will decay in fifty years is one half" can be said to be a true statement. But what is the fact that it corresponds to? Similarly, one may truthfully say that "If we could travel at the speed of light we would be able to reach the nearest star in less than five years". Nevertheless, the truth of this statement, according to the correspondence theory, would have to refer to a fact. What is this fact and where does it exist? The probabilistic and hypothetical statements are certainly about events, but they can hardly be said to correspond to such events.

Since the problem with the correspondence theory is connected with the theoretical element in statements, it becomes further exacerbated if, as we have seen, all scientific statements are theoretical in nature. This means that we cannot separate a class of theory-free statements from a class of theoretical statements, even though we can distinguish observation reports from other statements. Thus, the truth of a statement depends not only upon the nature of the world, but also on the other statements we believe about the world. This would not accord with the correspondence theory which requires us to suppose that the facts - and only the facts - are crucial to determining the truth-value of a statement.

The correspondence theory appears to ignore the relevance of other parts of our conceptual background in establishing the truth of a proposition. In particular, it seems to endorse a sort of straightforward connection between statements and the world that,

according to the Duhem-Quine and the Kuhn-Feyerabend theses, we cannot expect. We have seen that, if a statement appears to be disconfirmed by observation, we can always retain the theoretical statement if we make sufficiently drastic alterations in our conceptual background (either by modifying the observational theories used to make the report or the auxiliary theories used to deduce the prediction). The facts - whatever they are - cannot compel us to consider that a statement has been falsified, or that it does not correspond to the facts. If truth is correspondence to facts which exist independently of our conceptual framework it is difficult to see how this can be the case.

Furthermore, the correspondence theory of truth is difficult to reconcile with the finding of the sociology of knowledge. Our most general metaexpectations given in paradigms are constitutive of all our theoretical beliefs as well as our observation reports. Thus, our statements cannot be what they are only by virtue of a correspondence with so-called facts which exist independently of cultural influence or interests. The content of statements that are deemed to be true are as much dependent on these cultural factors as they are on the events they are intended to be about.

Neither is the correspondence theory easy to reconcile with the expectational view of statements and the holarchic theory of meaning. The expectational view shows that all statements are both analytic and synthetic. Hence any statement can be treated as if it were analytic and, therefore, true by definition. This is the point of Duhem-Quine and the Kuhn-Feyerabend theses. If truth is correspondence to a nonlinguistic fact then statements can hardly be dealt with in this fashion.

Also, the holarchic theory reveals that the meaning of a statement would change as a result of theoretical changes that do not require us to reject the statement in question. If truth is a relation between a statement and a fact only, and this relation exists by virtue of the meaning of the statement, it cannot be maintained that the statement remains true even after we make modifications to other parts of our conceptual framework. Only within the context of a theory of meaning which can isolate the meaning of a statement from the rest of the conceptual background in which it is embedded can we formulate a viable correspondence theory of truth.

Unlike the correspondence theory which confronts problems in dealing with the theoretical nature of all scientific statements, the coherence theory faces difficulties because of its inability to deal adequately with observation reports. This theory was first formulated by the great metaphysical idealists and rationalists like Leibnitz, Hegel and Bradley who held that reality constituted a coherent system. It was subsequently developed as an epistemological theory by some of the logical empiricists like Hempel and Neurath, and also by philosophers not allied to this tradition - e.g., Quine. According to the coherence theorists, to say that a sentence is true is to assert that it coheres with the system of other statements that we hold; and to test for the truth of a statement is to examine its coherence with this system of statements. Thus, coherence provides both the meaning and the criterion of truth. The coherence theory of truth, as held by the followers of the logical empiricist tradition, developed out of the critique of a foundationalist basis for knowledge. Early logical empiricists like Schlick and Carnap had held that there were certain

sentences - the so-called protocol sentences that were used to express the results of observations - which gave an incorrigible foundational basis for science.⁹ Theories were held to be supported, confirmed or refuted by appealing to this empirical basis. This position was criticized by Neurath:-

There is no way of taking conclusively established pure protocol sentences as the starting point of the sciences. No tabula rasa exists. We are like sailors who must rebuild their ships on the open sea, never able to dismantle it in dry-dock and to reconstruct it out of the best materials.

In unified science we try to construct a non-contradictory system of protocol sentences and non-protocol sentences (including laws). When a new system is presented to us we compare it with a system at our disposal, and determine whether or not it conflicts with that system. If the sentence does conflict with the system we may discard it as useless (or false)... One may, on the other hand, accept the sentence and so change the system that it remains consistent even after the adjunction of the new sentence. The sentence would then be called 'true'. The fate of being discarded may befall even a protocol sentence. No sentence enjoys the poli me tangere which Carnap ordains for protocol sentences.

Thus, according to Neurath's coherence theory, the truth of a statement - including observation reports - is to be judged, not by comparing it to experience, but by examining how it coheres with other statements. This statement may be related to other statements in that it may be logically derived from them, or at least not contradict them. Where there is a conflict the statement itself may be discarded, or the system of other statements may be adjusted to eliminate the conflict. Both the meaning and the criterion of truth for any statement is its coherence with a system of other statements.

The coherence theory of truth does not face the difficulties confronted by the correspondence theory as a result of the theoretical nature of all scientific statements. It does not require us to accord a

foundational status to observation reports - such reports could be corrected or discarded on the basis of theoretical statements. Nor does it require a separate ontological domain of facts that are distinct from statements and the world these statements refer to. Furthermore, the difficulties caused by the theoretical nature of general statements, and statements about the past and the future, do not arise for the coherence theory. Such statements can be deemed to be true because they cohere with the rest of our system of statements. The same may be said of statements of probabilistic laws or statements expressing conditionals.

The coherence theory of truth, however, confronts problems in dealing with that aspect of truth that the correspondence theory was intended to capture. It clearly recognizes that a statement can be said to be true by virtue of relations it has to other statements, but what is absent in the theory is the recognition that a statement is true also because it tells us something about a reality independent of our statements. Its model of theories is to view them as networks of logically connected propositions like an uninterpreted mathematical system.¹¹ Thus one can, given an axiomatic system like geometry without the parallel postulate, decide if it should be enriched by the addition of another postulate. The new postulate can be deemed acceptable if it coheres with the rest of the system; or alternatively, we may modify some of the postulates in order to accept the new one. Thus, e.g., Saccheri's geometry can be enriched by the addition of the parallel postulate to obtain Euclidean geometry; but to add the postulate of no parallels we also need to change the axiom that straight lines intersect at a single point. Only then can we obtain a consistent system - Riemann's geometry. In this context the holistic notion of

truth, so intimately connected with the coherence theory, appears adequate. All statements are on par, and have to be tested by their coherence to other statements.

However, a system may be coherent and, yet, not be about the world. This point has been tellingly made by Schlick.

If one is to take coherence seriously as a general criterion of truth, then one must consider arbitrary fairy stories to be as true as a historical report, or as statements in a textbook of chemistry, provided the story is constructed in such a way that no contradiction ever arises. I can depict by help of fantasy a grotesque, world full of bizarre adventures: the coherence philosopher must believe in the truth of my account provided I take care of the mutual compatibility of my statements, and also take the precaution of avoiding any collision with the usual description of the world, by placing the scene of my story on a distant star, where no observation is possible.¹²

According to Schlick the only way of avoiding this problem is to specify in advance those statements which are to be maintained independently of the coherence condition, and to which all other statements have to be accommodated. The statements having this special status are the observation reports.¹³ The same difficulty also leads Quine - who has been strongly identified with the nonfoundational view and the coherence theory - to assert that observation statements are, in some sense, incorrigible.¹⁴

We have seen that, even if observation reports do have a status different from the other statements of science, they cannot in any way be deemed to be incorrigible. Nevertheless, they do specify the data upon which scientific theories are constructed. Rescher has recognized this in developing his coherence theory of truth. He allows that any coherence theory must take into account the sense in which theories have to be coherent with statements that provide information about the world.

True theories have to be coherent with the data even if these data are recognized to be corrigible. Thus he writes:-

Any adequate coherence theory of truth must resolve the objection: Why should coherence indicate truth? Regardless of how we resolve the issue of defining truth, truth is determined by a relation of a proposition to actual facts. Coherence, on the other hand, is a matter of the relation of propositions to each other. How then can we pass from one to the other?¹⁵

He resolves the issue by introducing the concept of a datum. For Rescher, a datum is not a proposition that is true, but one that is a truth-candidate which can be presumed to be true if there are no countervailing reasons that are operative. Thus "a datum is ... a proposition that one is to class as true if one can, that is, if doing so does not generate any difficulties or inconsistencies."¹⁶

According to Rescher there are multiplicity of sources of data - first hand sense-perceptions, memory, vicarious observation reports, synthetic cognitive processes like assumptions, suppositions, conjectures and so on.

The same sort of coherence theory is espoused by Hesse who, like Rescher, wants coherence to provide the criterion of truth and correspondence the meaning of truth. Hesse recognizes that observation reports have a special status in any coherence theory that is not accorded to other statements. However, she views this is to be due to the temporal priority they are accorded in any judgments of truth - other statements are required to be coherent with most (but not necessarily all) observation statements, but not vice versa. This temporal priority, according to Hesse, does not require us to suppose that observation reports are given any epistemological priority. Thus, like Rescher, she answers the question "Coherence with what?" by

replying that it is coherence with a corrigible data base.¹⁷ What is clear from the above account is that no coherence theory is itself adequate without including, at least, some statements that are responses to events in the world. These statements have to be accorded some sort of priority that other statements do not have - for Rescher they are the ones that have to be classed as 'true if one can'; for Hesse they have temporal priority over other statements. Rescher and Hesse require this because they recognize that coherence, in itself, cannot be the meaning of truth though it is their criterion for truth. The data or the observation reports that are prior to other statements allow them to recognize that truth also involves a relation between a statement and the world. It is for this reason that they view the correspondence theory as furnishing the meaning, and the coherence theory as providing the criterion, of truth.

This solution, however, obfuscates the whole issue of truth. For as we have seen, even if we suppose correspondence to provide the meaning of truth, coherence need not furnish us with a criterion that leads us towards the truth. The view confronts another difficulty that is not easy to accommodate into its framework. This is the possibility that there could be more than one statement (or scientific theory) which coheres with the data or observation reports. The coherence of a number of theories with the same data would generate no problems for a coherentist view, but how can we allow that all these theories are true if we interpret truth to mean correspondence to the facts? This view can be maintained only if we suppose that the regulative principle correspondence provides can, in the long run, guarantee that there would only be a single theory that would cohere with the data. This is an

unproven assumption and, moreover, one that is not supported by all the historical evidence. In quantum mechanics, at present, we have two different theories - matrix and wave mechanics - that are both equally coherent with a large array of data. What reason do we have - other than adopting it as a mere supposition - that the world is such that, in the long run, there is only one theory that could be said to cohere with the data. Actually, even if we do manage to achieve a theory that is totally coherent with the data, it may turn out to be only one of several theories that are able to do this for us. Nevertheless, if we maintain that truth means correspondence how can we accept that all these theories correspond to the world without grossly distorting the notion of correspondence?

Another aspect of the problem with the coherence theory is connected with its excessive emphasis on the holistic dimension of truth.¹⁸ Given the coherence theory, we cannot talk of the truth or falsity of isolated statements or theories. According to the coherence theory statements or theories acquire the property of being true or false in relation to the whole corporate body of knowledge we have accepted. Nevertheless, scientists do speak of the truth of single statements, or theories, that are inconsistent with a large body of accepted knowledge. They may believe that these individual statements or theories are true without being able to say which of the rest of their knowledge system has to be modified in order to accommodate the true statement or theory.

Take the case of black-body radiation that confronted physicists at the end of the 19th century. Here was a particular observational result which did not agree with the body of theoretical knowledge contained in

thermodynamics, classical mechanics and classical electromagnetic theory. Given a coherence account of truth, these experimental results would have been rejected as false. Nevertheless, this was not done because, scientists felt, there were strong independent grounds for accepting the observational results even though they were anomalous to accepted scientific theories. The same sort of situation exists with respect to any of the anomalous findings of science. If coherence is used as a criterion of truth it is difficult to see how isolated anomalies to generally accepted theories can be recognized, or be allowed to persist.

None of the difficulties encountered by the two classical theories considered are faced by the pragmatic theory. This theory clearly recognizes that scientific theories are conceptual structures that do not merely reflect external reality. They prove their worth, not only in terms of their practical utility, but also in terms of their utility in serving human interests and desires. Pragmatists emphasize the cash value of theories as instruments for practical action as well as in embodying human interests. The pragmatic theory, therefore, captures most closely the dual aspect of statements as being both descriptive of the world, and prescriptive of how the world should be represented. It also enables us to understand how theories can lead to successful practical activity and reflect sociocultural values at the same time. Nevertheless, the theory confronts other difficulties most of which are centred on its inability to specify how the two dimensions of utility are to be balanced. An overemphasis on the utility of interest takes us away from the world of events; the emphasis on practical success does

not indicate how ~~its~~ success it, to be determined. We shall see that the representation theory enables us to capture the crucial insights of the pragmatic theory, without leading us into all the traditional pitfalls that make this theory so unacceptable.

6.3 THE REPRESENTATION THEORY OF TRUTH

What is ideally required is a theory of truth that would embody both dimensions of what is involved when a statement ~~is~~ said to be true. To do this we require a theory that would allow us to say that a statement is true both by virtue of a relation it has to the world, as well as the relation it has to other statements. I want to suggest that this can be achieved if we consider a statement to be true if it is able to represent acceptably events in the world. Whether a statement represents acceptably depends on whether it is part of a system of accepted theories, or whether it is a reportive statement made in the context of accepted observational theories. Thus, the definition of truth proposed rejects the view that truth depends on some sort of correspondence of a statement to the world. Truth is an epistemic notion and is closely intertwined with the way statements achieve epistemic acceptance.¹⁹

However, characterizing truth as acceptable representation is not quite adequate. For epistemic acceptance of a statement may be lost, whereas a true statement is supposed to remain true indefinitely. Many sentences that were once considered acceptable representations of events were later discarded as false. A statement like 'Atoms are indivisible' was acceptable in the nineteenth century but is no longer so. What this suggests is that we have to define a true statement as one that is not

only an acceptable representation now, but will remain so into the future. It is an idealized notion of acceptability that we want to attribute to truth. Thus:-

A statement is true if, and only if, it is an idealized acceptable representation of events.

Let us refer to such a statement as an ideal representation of events.

The notion of idealized acceptable representation can be formulated fairly precisely. We have seen that a theory is considered acceptable if its relative explanatory indispensability is large compared to all its rivals. This, however, is not sufficient grounds for supposing a theory to be true. A necessary condition for accepting a theory to be true is to require that its relative explanatory indispensability be infinite with respect to all its rivals. Even if this were so, it may nevertheless be possible that the theory may, at some time in the future, be overthrown. We would not be prepared to allow that the theory was true at one time but became false later. We want to say that it had always been false, but had been mistakenly surmized to be true at some time. For a theory to be true we require that it, not only have an infinite relative indispensability now, but that it also continues to possess the same degree of relative indispensability into the future. In this sense truth is a regulative ideal. We aspire towards theories that are seen to possess an infinite degree of relative indispensability in relation to their rivals at all times. It is the statements of such a theory, and the observation reports made in its context, that can be deemed to be idealized acceptable representations of events in the world. They furnish ideal representations of events.

The representation theory of truth is often proposed as an equivalent of the correspondence theory of truth. I want to argue that

the theory is distinct and quite different from the correspondence theory. As we saw earlier, we cannot develop a correspondence theory along the lines that a statement was true because it corresponded to the facts. Great difficulties are involved in giving a correspondence account of the truth of statements about the future or the past; or to deal with the truth of universal, probabilistic and hypothetical statements.

A representation theory of truth does not confront these problems. A statement could ideally represent the events even if it were a general statement; statements could ideally represent events that have happened in the past or are anticipated to happen in the future; a probabilistic statement could be employed to ideally represent a single event or a class of events; hypothetical statements could also be employed to ideally represent events. Also, most importantly, the theory-ladenness of a statement does not preclude it from being able to ideally represent an event. Thus, clearly, the notion of ideal representation cannot be identified with that of correspondence.

It is evident that a statement does not ideally represent an event merely by virtue of a relation it has to that event. It also does this by virtue of the relations it has to other statements. To recognize that a statement is true requires us to recognize that it bears a relationship to other statements we hold, as well as a relation to the event in question. We recognize a statement to ideally represent by virtue of its meaning. But the meaning of its terms have both a theoretical and an observational component. The statement ideally represents the event only in the context of the theoretical and observational meanings of its terms. Since the theoretical meaning is

dependent on the conceptual framework, and the observational meaning is given by empirical associations to experience, the statement can be seen to represent only because of the relation it has both to statements and to the world. Thus, the holarchic theory of meaning suggests that neither correspondence to something in the world nor coherence with statements about the world, can provide an adequate characterization of truth. The holarchic theory of meaning requires a representation theory of truth.

We saw that one of the basic problems in adopting a coherence theory of truth lay in its excessively holistic approach. It was not possible to talk about the truth of individual statements - only about systems of statements. This is not so with the representation theory. Individual statements can be said to be true or false because they do, or do not, ideally represent events. Observation reports, for example, made through observational theories that we accept as true, can also be accepted as true regardless of what other statements we may hold over and above these observational theories. Furthermore, it is not merely coherence with the observational theories that determines the acceptability of the observation report - it is also determined by what is perceived in the world through these observational theories. The observation report is seen to represent both by virtue of the relation it has to the world, and to the statements of the observational theories involved in constituting it. The report can be said to be true, not because it merely coheres with the observational theories employed in constituting it, or because it corresponds to an event given independently of these theories. It is true because it represents an event given in the context of a network of observational theories that

are considered true.

Thus, the problems encountered by the coherence theory in dealing with the issue of how statements can be about the world does not arise for the representation theory. Coherence with other statements is only one aspect of ideal representation; another is that the statements also refer to events by virtue of the observational meaning their terms possess. Thus, the synthesis attempted by some philosophers who have sought to make coherence the criterion of truth, with correspondence as the meaning of truth, is naturally integrated in the representation theory.

The representation theory furnishes us with both the meaning and the criterion of truth. It makes it possible to say that a statement is true means that the statement ideally represents events in the world. The criterion we use to test if the statement is true involves testing to see if the statement ideally represents the events in question. Since the terms of a statement possess both theoretical and observational meaning this test involves, not only examining the situation to see if the statement coheres with other statements, but also checking to see if the observational meaning of the terms allow the sentence to be linked to the events concerned. Thus the truth of a statement depends on a relation between the statement and other statements, as well as a relation between the statement and the world. The representation theory synthesises the two aspects of truth that the correspondence and coherence theories attempt to deal with separately.

The representation theory also allows scope for a pragmatic dimension to be embodied in true statements. All statements, being expectations, are not only claims that they can represent events but

also demands on how events should be represented. The prescriptive aspect reveals that interests may affect the content of true statements. This is especially important in understanding the content of the systems of expectations that constitute scientific theories. We saw that theories can be internally and rationally evaluated in terms of their ability to explain a domain of events. A successful explanatory theory may be considered to be true because it ideally represents the events in its domain. Nevertheless, the content of the theory is not explained merely by revealing that it is the most acceptable explanatory theory, or that it is able to ideally represent the domain of events concerned. We saw in the last chapter that to explain the content of scientific theories, as distinct from evaluating this content, we have to take into account sociocultural factors. Therefore, pragmatic factors can play a role in determining the content of true theories and true statements.

Pragmatic theories of truth attempt to capture this insight. They lead us to recognize that the content of true statements cannot arise merely because of a relation that exists between statements, or between statements and the world. We have also to take into account the relation that statement has to the individual accepting it. Thus James, for example, held that the truth of statement was to be determined by its utility to the individual holding it.²⁰ The notion of utility can be interpreted in one of two ways and James, at various times, tended to emphasize one or the other interpretation.

On the other hand, a statement has utility because it leads to successful predictions and thereby enables the individual to obtain control over the environment. On the other hand, it may have utility because it serves the interests of the individual - e.g., the belief in

the statement may offer psychological gratification. These two notions are logically independent, for ideas that lead to successful predictions may not offer psychological gratification, and vice versa. James himself recognized this. Hence he made numerous attempts to show that beliefs that do not lead to successful predictions cannot furnish psychological gratification. His arguments have generally been unconvincing and have been criticized by others, e.g., Lovejoy and Russell.²¹

There are numerous difficulties involved in accepting any pragmatic theory of truth. Though I do not wish to deal with these in any detail the main defects of the theory may be summarized as follows. It is a theory that may lead us to reject statements that we have independent grounds for accepting; it appears to allow us to accept contradictory statements as simultaneously true; it makes the notion of truth have an unacceptable dependence upon the observer; it does not provide any criterion for truth that transcends individual interests; and, finally, it does not allow conflicting debates about truth to be resolved if the parties involved have conflicting interests.²²

The great contribution made by the pragmatic theory, however, is not to our understanding of truth but to our understanding of the content of true statements. Pragmatism emphasizes the role that individual and sociocultural interests play in determining the content of statements that are accepted as true because they represent events in the world. Thus it refuses to let us forget that any proposition held to be true also embodies the practical and psychological interests of the individual accepting it. Thus, even if the truth of the content of a statement is established by the relations the statement has with other

statements and the world, the content of a true statement also involves a relation to the individual holding it.

This is possible because the requirement that a theory (or a statement) be true underdetermines the content of the theory (or statement). There could be a number of theories that are true because they are each capable of ideally representing a domain of events. This occurs, as we have seen, when two theories T and T^* are such that their relative explanatory indispensability with respect to each other is indefinite, but is infinite with respect to all other theories. This undetermination of content by the requirement of representation (or truth) allows pragmatic considerations to play a role in choosing between equally true theories. However, such pragmatic considerations are not criteria of truth. Insofar as we are concerned with truth - and truth alone - pragmatic factors cannot be employed. We cannot use them to reject, as false, a theory so long as it ideally represents the events in its domain. In fact, we must be prepared to accept a multiplicity of true theories, or pursue further investigation that might reveal that one or more of them do not represent all the events. However, in choosing between true theories we may use pragmatic considerations to determine which of the theories we wish to accept in practice. It is for this reason that pragmatists are right in supposing that utility plays a role in determining the content of true theories adopted in practice, even if they are mistaken in their view that utility determines the truth of adopted theories.

Another way of viewing the situation is to consider that theories which are true may also represent the interests of those who accept them. However, they are not true because they represent such interests.

The goal of internal rationality is to establish the truth of theories and to exclude all false theories. External rationality could invoke pragmatic considerations that may be employed to select the theory we wish to adopt, in a specific context, from the theories that can be said to be true. This is the reason why any externalist theory of rationality must take into account internal rationality as one of the factors that determines the content of accepted scientific theories. Insofar as we are concerned with the cognitive aspects of science we have to acknowledge truth as the fundamental goal of science. It is only after the truth of a theory has been established that we can allow interest to dictate the theories we are prepared to adopt. This, however, is not a process that occurs sequentially in time. We do not first determine a class of all possible true theories, and then select one of them to adopt on the basis of pragmatic values. What happens in practice is that we do not allow pragmatic factors to determine which of two competitor theories are to be preferred cognitively, even though pragmatic factors may have played a role in determining the content of these theories.²³

The representational theory of truth also enables us to interpret the progress of science as a direct progress towards the truth. This is not possible within the framework of either the correspondence or the coherence theories without employing highly artificial interpretations. Take the case of the correspondence theory. When a scientific revolution replaces a complex of theories by a new theory-complex we have to suppose that, if the new complex of theories corresponds to the facts, the original complex could not have done so. Furthermore, if we assume that these new theories themselves would be replaced in the

future, then they too cannot be said to correspond to the facts. We cannot view the change from one theory-complex to another as involving progress towards the truth. This is because theories either correspond to the facts or they do not - they cannot correspond more to the facts or less to them. Thus the movement from Aristotelian dynamics to Newtonian dynamics cannot be directly interpreted as progress because neither theory (if we assume Einstein to be correct) corresponded to the facts.

Within the coherence theory there is scope for using the concept of progress provided it is interpreted in terms of degrees of coherence. However, it is not clear that this would capture the notion of scientific progress. For, progress in coherence can be achieved by eliminating all the data that are inconsistent with a theory-complex. Classical mechanics is more coherent than the theories of relativity and quantum physics taken together. Nevertheless, one cannot say that these theories of physics were not progressive with respect to the classical theory.

The notion of progress towards truth is offered in a direct manner by the representation theory. We say that a scientific theory gets accepted only if its relative explanatory indispensability with respect to competing theories is very large. This means that a new theory-complex explains (nearly) all the events accounted for by an earlier theory or theory-complex. Thus, whenever revolutions occur, the events explained by the new set of theories include most of the events explained by an earlier theory-complex as well as a whole new class of events not explained by it. Every metamorphic scientific revolution involves the acceptance of theories that possess a greater relative

explanatory indispensability with respect to the theories they replace. Were we to reach a stage where the theories we accept have infinite relative explanatory indispensability with respect to all others, we have prima facie grounds for supposing that they could be true. Hence, the progress of science is inextricably linked to progress in representation. Even if the new theories should turn out to be false they, nevertheless, involve a progress in our ability to represent the events in the world. This implies that there can always be progress towards the truth even if we never achieve the truth. Thus, truth can be a regulative ideal for science even if one cannot guarantee success in reaching the truth.

6.4 Perspectival Realism

The representation theory of truth suggests that there can be a plurality of true theories. This raises a problem with respect to the relationship between theories and the world they purport to account for. If these theories are interpreted realistically does it mean that there are a plurality of worlds? Or should we say that each of these theories are different versions of the same world? Are the entities postulated by true theories real? Furthermore, by choosing which of the true theories we want to adopt in a particular context, can we determine what sort of entities exist in the world? A naive realist interpretation of the terms of a theory leads, from pluralism with respect to truth, to pluralism of worlds. Possessing the freedom to choose between theories we also seem to possess the freedom to decide what there is in the world - i.e., the freedom to create world models. Of course, we cannot create world models arbitrarily because not all possible theories are true, but

there is still the freedom to choose between theories that are true. Even this limited freedom appears excessive - it seems to indicate an idealistic conception of what the world is.

Such a problem does not arise in the traditional metaphysical realist view of theories. Here it is assumed that the world exists independently of our theories (and hence, our minds) and that there can only be one true theory of it. The entities postulated by the theory correspond directly to the entities that exist independently in the world. The terms of the theory refer to these mind-independent objects. We do not create the world - we merely discover it. Thus metaphysical realism involves three interconnected, but logically distinct, assertions. I shall refer to them as external realism, referential realism and truth monism.

External realism is the view that our theories are about an external world that exists independently of the theories we hold about it. True theories give an account of a mind-independent reality that pre-exists our act of theorizing. Theories are attempts to represent this reality, but they are in no way involved in creating it. This is often considered to be a minimal requirement for any realist position, but there are some philosophers who (as we shall see) have denied external realism without, thereby, relinquishing the claim to be realists.

Referential realism is another basic component of the metaphysical realist position. Referential realism involves the assumption that the entities postulated by true theories are real. The referential realist claim is logically distinct from the external realist one. Referential realism does not require us to suppose that the objects postulated by

true theories exist independently of our theories. For, if we suppose that the postulated entities come into existence as a result of adopting these theories, then referential realism can be affirmed without requiring external realism. Furthermore, as we shall see later, we can deny that the entities postulated by true theories are real, and yet allow that our theories are about a mind-independent external reality - i.e., we can deny referential realism without relinquishing external realism.

Metaphysical realism also presupposes truth monism - the view that there can only be one true theory of the world. We have seen that the underdetermination of theories by the observational evidence strongly suggests that there can be more than one true theory. Also given the representation theory of truth it is possible for there to be a plurality of equally acceptable ideal representations - i.e., a multiplicity of true theories of the world. However, if we relinquish truth monism, referential and external realism become incompatible. For assuming truth pluralism and referential realism we have to allow that, by accepting one or another of the true theories, we can change what real entities there are in the world. In doing this we seem to be able to change the world by a choice of our theory - i.e., we seem to deny that there is a world that exists independently of our theories. Possessing the freedom to select among different true theories we appear to possess the freedom to determine what worlds there are.

Such a conclusion has been adopted by Goodman:-

"... right world-descriptions and world-depictions and world-perceptions, the ways-the-world-is, or just versions, can be treated as our worlds... If all right versions could somehow be reduced to one and only one, that one might with some semblance of plausibility be regarded as the only truth about the only world. But

the evidence for such reducibility is negligible..."²⁴

Goodman denies, or rather is agnostic; on the issue of an underlying mind-independent reality. His reason for agnosticism is that we have no access to a world that is given independently of our right versions of it.

"We cannot test a version by comparing it with a world undescribed, undepicted, unperceived... We may speak of determining what versions are right as 'learning about the world', 'the world' supposedly being that which all right versions describe, all we learn about the world is contained in right versions of it; and while the underlying world, benefit of these, need not be denied to those who love it, it is on the whole a world well lost."²⁵

Thus Goodman's position essentially involves a denial of external realism - theories are not about a world that exists independently of our minds (his agnosticism notwithstanding). Instead he proposes a constructivist account of scientific entities. For him the stuff of which worlds are made - matter, energy, waves, phenomena - are themselves made along with the worlds that we make.²⁶ For him worlds are right versions of the world, and the entities postulated by right versions are made along with such versions. This is not an account that requires him to reject referential realism, since the entities referred to by true theories are real.

Goodman, of course, recognizes that his view does not mean that we can create worlds arbitrarily. These are constraints on such creations - the only worlds we can create are those that are 'true or right versions'.²⁷ This diminishes, somewhat, what would otherwise appear to be a totally idealist conception of worlds. True and right versions of the world have to be discovered as much as created. Thus, for Goodman, worlds are as much found as made; knowing is as much reporting

as remaking.²⁸

Goodman's constructivist account of the relationship of theories to the world seems inadequate to me for a number of reasons. In the first place, if one were to deny that there is an underlying world that all the versions are about, why is there any constraint on the construction of worlds? It would seem that we could construct any world that we wished provided, perhaps, that it is consistent. However, we have seen that not all consistent theories are true. The fact that even Goodman finds it necessary to place constraints on possible versions that could be worlds means that there must be something that constrains the construction of true versions. But by denying external realism Goodman has forgone appeal to a mind-independent reality that acts as a constraint.

Secondly, even Goodman's qualified constructivism is excessively idealistic and incompatible with the corpus of knowledge science has to offer us about the world. Our theories of astronomy and biology suggest that the mind is a recent evolution in a vast cosmos that predated its existence by billions of years. It seems grossly unreasonable to presume that, by making a choice between two possible alternative theories (both of which are true), we could determine (or change) the nature of the world. The same problem arises with respect to the past - can we alter what has happened by choosing the theory we wish to adopt with respect to the past? Such a conception appears to be required if we follow Goodman in asserting that the stuff of the world is made along with our right versions of the world.

An alternative account of the relationship of true theories to the world is offered by Putnam. A more careful examination reveals that it

is essentially similar to Goodman's - the difference is more one of emphasis than substance. Like Goodman, Putnam is agnostic with respect to external realism. Whereas Goodman seems inclined to dismiss an underlying world, Putnam tends to allow for it. Nevertheless, he agrees with Goodman that such a world has very little epistemological work to perform. Referring to Kant he writes:-

"He does not doubt that there is some mind-independent reality; for him this is virtually a postulate of reason ... Today the notion of a noumenal world is perceived to be an unnecessary metaphysical element in Kant's thought. (But perhaps Kant is right: perhaps we can't help thinking that there is somehow a mind-independent 'ground' for our experience even if attempts to talk about it lead to nonsense.)"²⁹

Putnam also agrees with Goodman that there can be more than one true theory of the world, and that what there is in the world is determined by the (true) theory we decide to adopt. He argues that there could be equally coherent, even if incompatible, conceptual schemes which would be able to accommodate all our experiential knowledge. This, he suggests, shows that there could be a plurality of true theories about the world.³⁰

He elaborates his pluralism by expounding, what he calls, an internal realist view:

"For an internalist like myself... signs do not intrinsically correspond to objects independently of how those signs are employed and by whom. But a sign that is actually employed in a particular way by a particular community of users can correspond to particular objects within the conceptual scheme of those users. 'Objects' do not exist independently of conceptual schemes. We cut up the world into objects when we introduce one or another scheme of description."³¹

Thus Putnam, like Goodman, acknowledges that what objects there are in the world can only be determined internal to a theory.³² By changing our theory we can change our account of what the world consists

of. Each true theory gives an account of a world of real objects. These theories together allow for a plurality of real worlds. Internal realism also leads to world pluralism. Like Goodman, Putnam recognizes that there are constraints on the sort of worlds that are possible.

"I shall advance a view in which mind does not simply 'copy' a world which admits of description by One True Theory. But my view is not a view in which mind makes up the world, either (or makes it up subject to constraints imposed by 'methodological canons' and mind-independent 'sense-data'). If one must use metaphorical language, then let the metaphor be this: the mind and the world make up the mind and the world."³³

It is clear that Putnam's internal realism is not substantively different from Goodman's constructivism. For Putnam mind makes up the world subject to certain constraints; for Goodman we create worlds subject to constraints. Their only basic difference appears to be Putnam's reluctance to give up an 'underlying world'. He explicitly assumes external realism when he talks of different true theories as 'cutting up' the world into objects in different ways. But what is this world that is being 'cut up'? Is it a 'real world' different from the worlds of real objects given by true theories? If what is real is deemed internal to our (true) theories, how can this theory-independent world be deemed real? If it is real, then how can the different worlds postulated by different true theories also be real? Either way Putnam's internal realism is incoherent.

However, if Putnam discards the notion of an underlying world his internal realism would be no different from Goodman's constructivism. Putnam himself acknowledges the similarity in their views.³⁴ In that case all the criticisms against Goodman's excessively idealist views also applies to internal realism. There appears to be no explanation for the constraints required in theory construction and, hence, our

freedom to create worlds; the view is incompatible with what our most dependable scientific theories tell us about the relationship of mind and the world. These theories do not tell us that 'the mind and the world make up the mind and the world'. They tell us that the world existed prior to the mind.

I want to propose an account of the objects postulated by scientific theories that captures some of the insights offered by Putnam and Goodman, but which does not lead to their unfortunate idealist conclusions. Like them, I want to say that the objects recognized in the world cannot be given except in the context of some theory or another. These objects have to be considered as both constructed and discovered; invented and found. This means that what objects are recognized in the world depends not only on the external world, but also on the mind of the investigator. However, the theory I propose does not require us to view these objects as real. The distinction between Putnam's internal realism and Goodman's constructivism is not substantive because, if only constructed worlds exist, the objects so constructed are real; also if our theories determine what is real, then what is real is also constructed by our minds. In both these cases we have to deny external realism - that our theories are about an external world independent of our minds. It is for this reason that both Putnam and Goodman seem inclined to reject an 'underlying world'. They do this in order to save referential realism - the reality of the entities postulated by true theories.

I want to suggest a theory that rejects a referential realist interpretation of the entities postulated by scientific theories, but accepts external realism. It assumes that true theories are

perspectives upon an external world that exists independently of our minds. However, I reject the Kantian view that this external world is a noumenal reality that is radically inaccessible to us. I want to say that we can have theories that represent the external world, and that true theories give us access to this external world. Only a correspondence theory of truth - which requires us to suppose that the objects postulated by our theories correspond to objects in the external world - leads to despair about true knowledge of this external reality. Such a pessimistic conclusion is unwarranted on a representation account of truth. The representation theory allows that there can be more than one true theory of the world. Thus, there could be a number of true perspectives upon the world. The objects given through each perspective are not real; nor are they pure constructs. They are, what I shall call, perspectival objects.

To clarify what I mean consider the following situation. Take the vase/faces figure familiar to gestalt psychologists. Given the interpretation that it is a figure of two faces we perceive a certain set of objects - 'noses', 'foreheads', 'chins'. Given the vase interpretation we can recognize 'sides', 'a mouth' and 'a base'. The objects we perceive exist only internal to the interpretation we adopt. They have no independent existence. But the interpretation itself does not construct these objects; they arise also as a result of something that exists independent of the interpretation. Thus, the objects are neither only constructed or only discovered. They result from adopting a certain perspective on an independently given figure - they are perspectival objects.

A perspectival object is one that is neither constructed by the mind alone, nor exists independently of the mind. It arises as a result of the interaction of the mind and a mind-independent external reality.

Take a cone. It may be seen as a triangle or a circle depending on our perspective. But this does not mean that the 'triangle' exists independently of the mind, or that it is ~~purely~~ a construct of the mind. We can say of the triangle what Gorman and Putnam want to say of the entities postulated by true theories - that it is both constructed/discovered and invented/found. It is a perspectival object.

Perspectival objects possess a dual aspect that makes it possible to interpret them as theory-independent or theory-dependent. On the one hand, we can consider them to be the way we represent to ourselves the external world - we conceive the world as being constituted by such objects. On the other hand, we can treat them as the way the external world presents itself to us - we perceive the world as made up of such objects. The former emphasizes the mind-dependent constructivist aspect of these entities; the latter the mind-independent realist aspect. However, since perception is structured by conception and conception is supported by perception, neither a realist nor a constructivist account of them is adequate. Both aspects have to be accepted - which is why we have to treat these objects as perspectival.

Even the objects offered by false theories are perspectival. For example, when a Ptolemaic astronomer perceived the sun rising in the east he took this perception to be a veridical one. What he perceived depended both upon the external world and the theory he held about this world. However, a Copernican astronomer takes the perception of the rising sun to be an illusion. What he perceives is the horizon sinking

away from the sun as the earth spins. Thus, whether a perception is illusory or veridical depends on the kind of theories we hold about the world. I want to say that, similarly, whether a perspectival object is veridical or illusory depends on whether our theory is true or false. The entities postulated by true theories are veridical perspectival objects; those given by false theories are illusory objects.

The basic difference between true and false theories is that true theories can be used as observational theories to structure experience, and as explanatory theories to represent the results of experience, without a mismatch arising between perception and conception. This is not the case with false theories. False theories are recognized as false because, when they are used as observational theories, they give rise to experience that is not compatible with acceptable explanatory theories; or used as explanatory theories, they are not compatible with experience obtained through acceptable observational theories. True theories structure our experience of the world, and explain how the world is given to experience, in such a way that perception and conception match.

This explains how true theories and the entities they postulate relate to the external world. True theories are a system of interpretive categories through which the world may be perceived, and a system of representations for what is perceived, such that what is perceived is ideally represented by what is conceived. Thus, true theories do not merely copy the world - they structure the world into objects for us, as much as the world determines the possible ways in which it may be structured, so that perception can fit conception. The way the world can be ideally represented as a structure depends, not

only on the nature of the world, but also on the theories through which we approach the world. Since the objects in terms of which the world is given structure depend both on the world and our theories, these objects are perspectival. A true theory of the world offers us a perspective on the world.

We may describe the situation as follows. There is an external world that exists independently of our minds and our theories are about this world. This world can only be experienced through representations of it. Our representations are tested against our experience of the world, and our experience is obtained through our representations. I.e., we perceive the world through concepts that are supported by our percepts. Metaphysical realism supposes that there can only be one conception of the world that can ideally account for our perception of the world. Truth pluralism requires us to assume that there could be a number of conceptions of the world such that the perception of the world obtain through any one of them supports that conception. Thus the world lends itself to being represented by a number of true theories - true theories being those which allow us to represent the world in ways such that the experience obtained through these representations makes the representations ideally acceptable.

Each of these true theories allows us to experience the world in a different way. Each experience supports the theory that structures it. The entities postulated by the true theory are veridical - but they are neither only real nor only constructed. They are perspectival and internal to the theory. The world can be cognized as made up of these entities, and these entities may be perceived in experience, but this does not give them a mind-independent existence or make them mind-

dependent only. The world determines the kind of theories that can be said to be true representations of it; and the true representations we select determine the kind of entities in terms of which the world may be represented and experienced.

The view that I have proposed can be termed perspectival realism. It is realist because it affirms that there is a mind-independent external world. It is perspectivist because it denies that there can be only one true theory of the world. Each true representation of the world offers us a true perspective on the world. The external world determines what possible perspectives can be true. We are free to determine which of the true perspectives we want to adopt in any context. Independent of all perspectives we cannot represent the world or experience it. The entities postulated in each representation are neither real nor constructed - they embody both aspects and, therefore, have to be considered perspectival. Thus, perspectival realism accepts external realism but rejects referential realism.

At this point the reader may wonder if there is any need to postulate an underlying world. All that is offered to us in perception and conception are perspectival objects. Why not merely reject altogether the notion of a world that transcends what is offered to us by true theories and the perspectival objects they postulate? Such an objection, against an underlying world, is certainly telling against Putnam's internal realism. He supposes that the entities postulated by true theories are real, and that together they make up a real world. Thus his position does not seem to require, and actually is incompatible with, assuming such an underlying world. This is not the case with the perspectival realist viewpoint.

Perspectival realism rejects referential realism. The entities postulated by true theories are perspectival - they possess the complementary aspects of being mind-independent and mind-dependent. For such entities to be possible we have to assume an external world that transcends our conceptions and perceptions: a world that we cognize as made up of different perspectival entities as we adopt different true perspectives upon it. Thus, unlike Putnam and Goodman, who may well do without external realism, perspectival realism requires external realism because it denies referential realism.

The basic virtue of perspectival realism, as contrasted to the internal realist view, is that it does not require us to suppose that when we change from one true theory to another we are actually changing the world. We are merely altering our perspective on the world. There can be a plurality of true perspectives on the world and each of these involves seeing the world as populated by different perspectival entities. These entities do not exist independently of our perspective; neither are they only invented by our perspective. Their existence depends both on the world that transcends our perspective and the perspective we adopt upon this world.

There is a crucial problem that internal realists and constructivists seem to have overlooked merely because they confine themselves to the problem of a single isolated observer deciding which of a number of different true theories to accept. However, we live in a social world where different people may decide to adopt different true theories. Consider the case of two persons A and B each of whom accepts different, but true, theories of the world. Assume that A supposes that the world is made up of entities X, and B presumes that it is composed

of entities Y. If we assume referential realism it may turn out that to say the world is made of X is incompatible with asserting that it is made of Y. Since Goodman and Putnam accept referential realism they would have to conclude that A's world is made of X and B's world is made of Y, and that, A and B are in different worlds. However, if they are in different worlds, how can it be possible for A to see B as existing in his world and to be made of entities of sort X? Clearly we cannot deny the B is made of entities of sort X, since the theory A holds is presumed to be true. This is extremely paradoxical: to assume that B exists in A's world is to see him as made of X, and yet B exists in a world that is composed of Y.

These difficulties vanish the moment we reject referential realism. Given the perspectival realist view we can assert that both A and B are in the same world, but that they have adopted different true perspectives on the world. In one perspective B is seen to be made of X, and in other of Y. Even though both perspectives are radically different they are nevertheless true. They are true because they are both idealized acceptable representations of the world. In fact, there is no need to assume that A and B have to even disagree - they could each accept both perspectives as mutually exclusive, but simultaneously true. (Thus the perspectives involved in seeing a gestalt figure as an old or a young woman could be mutually exclusive and simultaneously true.) In different situations A and B could adopt either one of the perspectives in the same way that scientists, in different situations, adopt the wave-formulation of Shrodinger's quantum mechanics or Heisenberg's matrix mechanics.

This reveals even more clearly that the acceptance of external realism, and the denial of referential realism, is not redundant to the perspectival realist view. To save referential realism in the context of truth pluralism is to make it impossible to accept all true perspectives upon the world. Unlike the metaphysical realist, Putnam and Goodman are pluralist with respect to truth, but monist in requiring that we are allowed to accept only one true theory. Perspectival realism takes more seriously than either of them the pluralist conception of truth. It allows us to accept all true theories as different true perspectives upon an external world. It does not require us to confine ourselves to only one perspective, or to consider that someone who adopts a different true theory has necessarily excluded himself from our world. Though the external world cannot be known independent of a perspective, and though it transcends all our perspectives, the discovery of more true perspectives is a discovery that enhances our awareness of the world. It is not a discovery that separates and insulates us into different worlds.

Summarizing, we may say that perspectival realism affirms that there is an external world independent of our theories; that there can be a number of true theories of this world; and that the entities postulated by true theories are neither real nor constructed - they are perspectival. It denies the metaphysical realist view that there can only be one true theory, and that the entities postulated by this theory are real. It rejects the Putnam-Goodman view that there is no need to appeal to a world independent of our theories, and that the entities postulated by true theories are real. It refuses to accept the Kantian view that there can be only one true theory, and that the world

independent of our theories is a radically inaccessible noumenal reality. Kant was led to postulate a noumenal reality because he assumed that the categories which are constitutive of all experience were not supported by experience. Hence, these categories could only be viewed as distorting lenses through which a noumenal reality was given as phenomenal. We have seen, however, that experience is structured by observational theories which have themselves been tested against experience. Thus the categories through which we acquire experience are themselves supported by an appeal to our experience of the external world.

The perspectival view of the entities postulated by science accounts for many of the dualisms characteristic in traditional epistemologies. If these entities reveal a double aspect of being constructed and discovered then we cannot make a distinction between methodological and ontological principles. The former specify the sort of procedures we have to adopt to arrive at acceptable theories; the latter the sort of entities we deem to exist in the world. If the sort of entities we recognize in the world are dependent on the theories we adopt about the world, methodological principles are also implicitly ontological in nature. The perspectival view of the entities recognized in the world requires us to reject the dualism of methodology and ontology. Such principles - as we saw earlier - are both prescriptive and descriptive. Objects being perspectival, these principles are metaexpectations.

Also, if the entities referred to by theories are perspectival then all observation is theory-laden. What is experienced in the world is affected by which perspective we adopt on the world. We cannot

represent and experience the world independent of any perspective. We have seen that the theory-ladenness of experience requires us to give up the dichotomies prescriptive-descriptive, analytic-synthetic and theoretical-observational with respect to statements. We also need to conceive of truth as ideal representation rather than correspondence or (ideal) coherence. Hence, the perspectival realist view requires us to give up many dualisms characteristic of traditional philosophies.

CONCLUSION

The theory of knowledge I have outlined resolves many of the difficulties inherent in the traditional views. The atomic empiricist approach has had a long history that began with Locke and Hume, and culminated in logical empiricism and its offshoots. The holistic empiricist approach is of more recent origin, though its roots are traceable to the idealists and conventionalists of the nineteenth century. Holistic philosophers have emphasized what Suppe has characterized as 'Weltanschauungen' positions. Contributions to this tradition have been made by many renowned philosophers - Quine, Sellars, Hanson, Toumin, Kuhn, Feyerabend, Putnam and Goodman. Speaking metaphorically we may say that the atomic empiricists adopt a mirror image of knowledge. For them knowledge is an attempt of the mind to reflect reality as it is. By contrast, the image of knowledge emphasized by the holistic empiricists can be loosely characterized as that of the web. For them knowledge is an attempt to acquire a coherent account of the world.

In place of these, I have offered the image of the gestalt as the ideal metaphor in order to deal with the problems of epistemology. Where the mirror involves attempting to perceive the world uncontaminated by language, and the web involves viewing knowledge as a network of propositions, the gestalt leads us to recognize that knowledge is both dependent on language and a world independent of language. To facilitate discussion I shall give an account of what may

be regarded as idealized descriptions of the mirror and web views of knowledge. This is not to suggest that there is any one philosopher who would endorse all components of these idealized viewpoints, but it will, at least, give us a way of contrasting the images offered by atomic and holistic empiricism with that of gestalt empiricism.

The atomic empiricist begins with sense-data as the foundational units of experience. Observation reports are direct descriptions of sense-data, or complexes of sense-data. The terms of such reports - the so-called observation-terms - are given meaning atomistically by empirically associating them to given sense-data complexes. This descriptive view of statements and atomic theory of meaning leads the atomic empiricist to a correspondence theory of truth - a statement is true because it corresponds to a fact: a fact being that sort of thing which is directly given to experience. Observation reports, being responses to given experience, are immune to sociocultural influence. Also the conventions of language, being directly associated to experienced sense-data, are immune to empirical disconfirmation.

The atomic empiricist position has always confronted difficulties with theoretical statements that are not observation reports. Such statements could not be directly seen as descriptions of sense-data. The terms of these theoretical statements could not be given meaning solely by means of empirical associations to sense-data complexes. Since different theories could explain the same set of observation reports it was difficult to give an account of true theoretical statements in terms of the correspondence theory of truth. Furthermore, the underdetermination of theoretical knowledge by appeal to experience made it difficult to explain how it could be said to be uncontaminated

by sociocultural influence.

Nevertheless, one may view the atomic empiricist program as, at its deepest level, an attempt to show how theoretical knowledge could ideally approach the knowledge offered by observation reports. Thus the problems that confronted atomic empiricists were to explain how theoretical statements could be said to be descriptive, how theoretical terms could be defined in an observation language, and how the truth of these statements could be interpreted in terms of correspondence to states-of-affairs that existed independently of them. Thus, the atomic empiricist program was also concerned with explaining how theoretical scientific knowledge could be said to be immune to sociocultural contamination - i.e., to give a purely internalist account of the content of scientific theories.

Of course, the atomic empiricists were never able to sustain this program completely. The history of this program is a history of concessions to the pressure of nonreportive theoretical knowledge. Newtonian induction from the phenomena had to be relinquished for a hypothetico-deductive model; an observational language of sense-data had to be replaced by a language of physical objects; the elimination of theoretical terms as 'unobservables' had to be replaced by a definition of them by means of correspondence rules to the observation language; and a correspondence account of theoretical truth was superseded by an instrumentalist view of such knowledge. But even such concessions become unworkable the moment it is realized that the observational language towards which they seek to approximate theoretical knowledge is itself theory-laden.

The holistic empiricist begins with language as the medium through which all experience is acquired. Hence, holistic empiricism denies that there is any foundational experience. All observation reports are also theoretical because they appeal to an experience intimately constituted by our theories. Even terminological meanings have to be instituted by an appeal to experience that is radically constituted by our conceptual framework. This leads holistic empiricists to deny that we can make a distinction between theoretical and observational terms; or a distinction between descriptive and prescriptive, or analytic and synthetic, statements. It also leads them to a holistic theory of meaning and an account of truth in terms of coherence. In the ultimate analysis it drives them towards a position where meaning, truth, and observation reports have all to be understood only relative to a conceptual framework. This relativisation of knowledge to a framework forces holistic empiricists towards a purely externalist account of scientific rationality, and a realism that can be sustained only internal to a theory.

The difficulties confronted by the holistic view can be traced to their inability to deal with observation reports. They see knowledge as a web of propositions only weakly linked to the external world. Though they acknowledge that there are constraints on the construction of the web, the web may be constructed in many different ways and any sort of specification of the constraints independently of the web is deemed to be impossible. In fact, it is the impossibility of giving an account of the constraints that leads them to a position which ultimately construes that worlds exist only internal to conceptual frameworks, and that different conceptual frameworks are incommensurable.

Speaking loosely one may view atomic empiricism as an attempt to move towards a language that is totally constrained by experience, and holistic empiricism as the endeavour to strive towards experience that is totally constrained by language. The former has a tendency to make conceptual knowledge a mirror that reflects reality; the latter has the tendency to take reality as that which is given by a web of propositions. To interpret knowledge in terms of the gestalt metaphor takes us beyond the mirror and the web. The mirror metaphor allows us to recognize that knowledge is constrained by something that transcends language. ~~The web metaphor enables us to recognize that knowledge is~~ crucially affected by language, without indicating in what sense it is about anything that transcends language. Only the gestalt metaphor reveals that knowledge is both crucially determined by language and about a world that transcends language.

The gestalt empiricist view reveals that there is a complementary dependence in which language both creates and describes experience. ~~The fundamental unit of experience is the gestalt which is neither merely~~ sense-data nor a pure creation of language. The gestalt is a linguistically structured pattern of given sense-data - something that depends both upon language and the world. Thus, gestalt empiricism requires a view of statements as possessing the complementary features of being prescriptive and descriptive at the same time. It requires us to see terminological meaning as having an observational and a theoretical component. It offers a representation theory of truth that allows us to affirm that the truth of a statement has a double dependence on language and the world. It denies that there can be either only a purely internalist or a purely externalist account of

science. The content of scientific theories can be evaluated internally, but has to be explained externally.

In a certain sense both atomic and holistic empiricism can be seen as limiting cases of gestalt empiricism. If we deny that what is experienced is theoretically structured patterns of sense-data, but affirm that only sense-data are given to experience, then the gestalt viewpoint reduced to that of ideal atomic empiricism. If we affirm that what is experienced is theoretically constituted, but deny that there are any given sense-data; the gestalt viewpoint reduces to that of ideal holistic empiricism. Thus, both ideal atomic empiricism and ideal holistic empiricism can be seen as extremes that arise when we refuse to recognize the double dependence of experience on language and sense-data. Pursuing the analogy we proposed in the introductory chapter, we can say that atomic empiricists are like physicists who seek to interpret all matters in a corpuscular framework - the unit of experience is atomic sense-data and the unit of language is atomistically given meanings. Holistic empiricists bear a resemblance to physicists who seek to interpret all matter in a wave (or field) framework. Just as the field is spread through all of space, so for the holistic empiricists, experience and meaning is determined by the totality of the framework we adopt. The gestalt view, in analogy with the quantum view, recognizes that experience has atomic and holistic aspects; and that the terms of a language have an atomic and a holistic component of meaning.

Furthermore, though all matter possesses both wavelike and particle-like properties, the former are most apparent only when the wavelength of the body is large compared to the detection device; when

this wavelength is small the body only reveals essentially particle like properties. Similarly, atomic empiricism provides a useful approximation for common sense knowledge where there is little theoretical disagreement and a close tie to observation reports; holistic empiricism appears very suggestive in dealing with largely theoretical knowledge which has only a weak link to observation reports. Thus most of the difficulties that atomic empiricists confront are connected with statements that are not observation reports; the difficulties confronting holistic empiricists centre a great deal around giving a credible account of observation reports. It is for this reason that atomic empiricists often appeal to common sense examples to support their position; whereas holistic empiricists tend to base their case upon the highly theoretical knowledge of science. However, just as no material entity is ever only a particle or only a wave (even when it can be approximated in certain situations to one or the other), we cannot assume that any kind of knowledge (commonsensical or scientific) can be more than approximately accounted for by either atomic or holistic empiricists.

Perhaps, the most important consequence of the gestalt empiricist view is that it dissolves two of the deepest presuppositions of the epistemologies introduced in the seventeenth century - the first being the view that rationalism and empiricism are radical alternatives, and the second being that knowledge has a foundation. Empiricist foundationalism was based upon the idea that all that was given to experience were the sense-data. Rationalist (conceptual) foundationalism was based upon the idea that there were truths which were given independently of experience - the intuitions of Descartes,

the synthetic a priori categories of Kant, the conventions of Poincare, and the analytic truths of the logical empiricists.

The gestalt view of experience undercuts all variants of empiricist and rationalist foundationalism. It requires us to recognize that experience and theory cannot be separated: we acquire experience through theories that are themselves supported by experience. The categories we impose upon the world to acquire experience are themselves supported by an appeal to experience. Thus there cannot be a foundational experience that can be used to support all knowledge. Similarly it also requires us to relinquish all rationalist foundationalism - including that attenuated form of rationalism that supposes that we can, by free choice, make a foundational system of linguistic conventions. All linguistic conventions are empirical claims: we cannot divide statements into those that are analytic and those that are synthetic.

At the same time we cannot relinquish the distinction between rationalism and empiricism. Insofar as theories are used to structure experience they are employed as categories imposed upon the world of experience and sustained by the rational intellect; insofar as they are tested against experience they are treated as empirically corrigible. This dual aspect of being both rational and empirical is a feature of all theoretical knowledge. This means that we cannot give an account of knowledge in purely rationalist or purely empiricist terms - knowledge exhibits the complementary aspect of being rational and empirical at the same time. This complementarity cannot be expurgated by separating the rational from the empirical, as Kant attempted to do - viz. by separating incorrigible, rational structuring principles from corrigible

empirical knowledge. Neither can one exercise this complementarity by denying the utility of the distinction between rational and empirical in the way Quine denies the analytic-synthetic, or Kuhn the prescriptive-descriptive distinction. The point is not to deny the complementarity but to live with it - and to recognize it as a complementarity that is an intrinsic feature of nearly every other epistemic category.

REFERENCES AND FOOTNOTES

Introduction

1. Gregory (1970); (1974)
2. Piaget (1952); (1955)
3. Hubel and Wiesel (1962, 1968) showed that there exist inbuilt mechanisms in animals which respond selectively to different contours. By examining how single-celled units in the visual (striate) cortex of cats and monkeys responded to visual stimulation by slits of light, edges between bright and dark surfaces, and dark bars, they demonstrated differential responses in different cell units. A detailed account of these, and other studies, on the role that physiological mechanisms play in perception is offered in Vernon (1970) pp.8-26.
4. Whorf, B. L. (1956)
5. Galileo (1958) p.328
6. This relativism is most notable in the views of Kuhn and Feyerabend. Sociologists of knowledge like Bloor and Barnes espouse similar conclusions.
Kuhn (1970a); Feyerabend (1975); Bloor (1976); Barnes (1977)
7. Quine (1951)
8. Laudan (1977) pp.142-145; Popper (1965) p.112

Chapter 1

1. Russell (1961) asserts:

"That there must be a pure sense-datum is, I think, a logically irrefutable consequence of the fact that perception gives rise to new knowledge"

..."It is because I regard single observations as supplying our factual premisses, that I cannot admit, in the statement of such premisses, the notion of a 'thing' which involves some degree of persistence, and can, therefore, only be derived from a plurality of observations." p.124 and p.135

This has actually been a problem for all foundationalist views that begin with sense-data - what grounds are there for supposing there to be an external world if all knowledge is knowledge of

sense-data? One of the most ambitious and coherent attempts to develop phenomenalist foundationalism was that of Carnap (1928).

Carnap subsequently modified his position to a physicalist foundationalist one. Carnap (1966)

2. This has been emphasized by Hanson. Referring to the faces/vase figure he says:-

"But while I see a Venetian goblet, you may see two men staring at each other. Have we seen different things? Of course we have. And yet if I draw my cup for you, you may say, 'By Jove, that is exactly what I saw, two men in a staring contest.' Or I may shift my attention from the cups to the faces. Does my retinal reaction shift? Do my sense-data change? There is nothing in sense-datum theory to suggest that my sense-datum, i.e., 'look(s)' of (the figure) does change. For clearly my private visual field is taken up with the same configuration of lines when I say I see a cup as it is when I say I see two different faces. And yet it would be absurd to say that I saw the same thing in both cases." Hanson (1969) p.93

3. Hanson (1958) Chap. 1; (1969) Chap.5

Kuhn (1970a) especially Chap. X.

Feyerabend (1962) pp.35-36

4. This becomes clear if we distinguish what is sensed from what is perceived. Gregory makes this point:-

"Perception is a matter of reading non-sensed characteristics of objects from available sensory data ... We not only believe what we see; to some extent we see what we believe." [My emphasis] Gregory (1970) p.15

5. Gregory (1970) p.39

6. Hanson sometimes appears to imagine that there is no distinction between a visual experience and an interpretation. He argues:-

"A blind man cannot see how a timepiece is designed, or what distinguishes it from other clocks. Still he may see that if it is a clock at all, it will embody certain dynamical principles; and may explain the action to his young apprentice. The latter, however keen his vision, can only describe the perturbations of the clock..."

Hanson (1958) p.59

7. Feyerabend (1965) pp.212-214

8. Duhem (1962) p.218

9. Hanson (1958) pp.5-8

10. Thus Kuhn writes:-

"...scientists are right in principle as well as in practice when they treat oxygen and pendulums (and perhaps also atoms and electrons) as the fundamental ingredients of their immediate experience... compared with these objects of perception, both metre-stick readings and retinal imprints are elaborate constructs to which experience has direct access only when the scientist, for the special purposes of his research, arranges that one or the other should do so. This is not to suggest that pendulums, for example, are the only things a scientist could possibly see when looking at a swinging stone. (We have already noted that members of another scientific community could see constrained fall.) But it is to suggest that the scientist who looks at a swinging stone can have no experience that is in principle more elementary than seeing a pendulum." Kuhn (1970a) pp.127-128

Hanson, too, expounds a similar position Hanson (1958) p.59. For a critique of this view of Hanson's refer to Brown (1977) p.87

11. Kuhn (1974) pp.472-473

12. Feyerabend (1975) pp.121-139

13. Sellars (1956) reprinted in Morick (1972) p.105

14. Sellars (1963) p.160

15. Kuhn (1970a) pp.150-153; Feyerabend (1970a) pp.219-229
16. Dijksterhuis (1969) p.34
17. Reichenbach (1958) p.103
18. Nagel (1961) pp.94-96
19. The view that co-ordinative definitions have empirical content has been argued by Shaffner (1969). This position is also endorsed by Suppe (1974) pp.102-109
20. For a more elaborate discussion of this issue see Papineau (1979) pp.7-10
21. This is also the claim embodied in the Sapir-Whorf thesis. As Whorf propounds it:-

"We dissect nature along lines laid down by our native languages. The categories and types that we isolate from the world of phenomena we do not find there because they stare every observer in the face; on the contrary the world is presented in a kaleidoscopic flux of impressions which has to be organized by our minds - and this means largely by the linguistic systems of our minds. We cut nature up, organize it into concepts, and ascribe significances as we do, largely because we are parties to an agreement to organize it in this way - an agreement that holds throughout our speech community and is codified in the patterns of our language...This fact is very significant for modern science, for it means that no individual is free to describe nature with absolute impartiality but is constrained to certain modes of interpretation, even while he thinks himself most free." Whorf (1956) pp.213-214
22. Hesse (1974) pp.17-19
23. Quine and Ullian (1978) pp.12-20. It may appear surprising that Quine, who is so closely associated with holistic and nonfoundationalist views of knowledge, should make these requirements of observation reports. Actually Quine makes contrary claims elsewhere, and this equivocation on his part will become more evident when we discuss his view on meaning in chapter 3.
24. Poincare (1952) pp.89-107
25. *ibid.* p.50

26. . ibid. p.73
27. Reichenbach (1958) pp.78-81
28. Nagel (1961) p.264 footnote 18
29. Mill, John Stuart (1843) Book II Chapters 5, 6 and 7
30. Ayer (1936)
31. Poincare (1952) p.138; (1958) p.110
32. Poincare (1952)
33. Nagel (1961) pp.201-202
34. Shapere (1966) pp.53-56 presses Feyerabend to say whether all changes of theory, however minor, are to count as meaning disrupting. Where he does respond to such criticisms Feyerabend reveals that his holism is somewhat less than total - only changes in fundamental laws are regarded by him as affecting meanings.
Feyerabend (1956b) p.259
35. Quine (1951) in Harding (ed.) (1976) p.59
36. Feyerabend (1975) ch.5
Poincare (1952) p.140

Chapter 2

1. Nagel (1961) p.448 defends this view even for the social sciences; he appears to take it for granted when it comes to dealing with the natural sciences.

"It is ... generally acknowledged that in the social sciences there is nothing quite like the almost complete unanimity commonly found among workers in the natural sciences as to what are matters of established fact, what are reasonably satisfying explanations (if any) for assumed facts, and what are some of the valid procedures in sound inquiry. Disagreements on such questions undoubtedly occurs in the natural sciences as well. But... except in areas of research that impinge upon moral or religious commitments, such disagreement is generally resolved with reasonable dispatch when additional evidence is obtained or when improved techniques of analysis are developed."

2. Rudner (1953)

Kuhn (1970a)

Feyerabend (1975)

Bloor (1976)

Barnes (1977)

3. This view is expressed most eloquently by Nietzsche in aphorism no. 3 in "On the prejudices of philosophers" in Beyond Good and Evil:

"Having kept a close eye on philosophers and read between their lines for a sufficient length of time, I tell myself: the great part of conscious thinking must still be counted among the instinctive activities and this is so even in the case of philosophic thinking... Just as the act of being born plays no part in the procedure and progress of heredity, so 'being conscious' is in no decisive sense the opposite of instinctive - most of a philosopher's thinking is directed and compelled into definite channels by his instincts. Behind all logic, too, and its apparent autonomy there stand evaluations, in plainer terms physiological demands for the preservation of a certain species of life."

4. A psychoanalytic critique of modern science is offered by Norman O.

Brown in Life Against Death: The Psychoanalytic Meaning of History

pp.314-316. A more favourable psychoanalytic perspective on

science is Feuer's (1963) The Scientific Intellectual: The

Psychological and Sociological Origins of Modern Science.

Koestler's The Sleepwalkers is a masterpiece of psychohistoriography that deals with some of the great founders of modern science - Copernicus, Galileo and Kepler

5. The economic interpretation of the natural sciences was not particularly emphasized by Marx, and it is not clear whether he would have included the physical sciences as a part of the ideological superstructure that he considered was erected upon economic interests. It was really Engels who developed this interpretation explicitly, and even went so far as to suggest that the technical needs of society played a far more crucial role in the development of science than ten universities. However, as Basalla (1968) p.x notes, the Marxist interpretation acquired prominence only after the 1930's. A very comprehensive survey of Soviet Marxist views on philosophy of science can be found in Graham (1972)

6. Stromberg (1975) Philosophy of Science in the Soviet Union pp.25-48

7. Kuhn (1970a) p.161; Feyerabend (1975) Chapter 18

8. Barnes and Bloor openly embrace a relativist position.

"The relativist, like everyone else, is under the necessity to sort out beliefs, accepting some and rejecting others. He will naturally have preferences and these will coincide with those others in his locality... he accepts that none of the justifications of his preferences can be formulated in absolute or context independent terms. In the last analysis, he acknowledges that his justification will stop at some principle or alleged matter of fact, that has only local credibility" "Relativism, Rationalism and the Growth of Knowledge: in Hollis and Lukes (eds) (1982) p.27

This is a far cry from what Merton (1938) has characterized as the sentiments embodied in the ethos of science - "intellectual

honesty, integrity, organized skepticism, disinterestedness and impartiality" P.259 in reprint in The Sociology of Science. For a general critique of cultural relativism refer Bernsen (1978) p.184-205

9. There are many sociopsychological historical studies of science - Caneva (1978); Farley and Geison (1974); Forman (1971); Frankel (1976); Jacob (1972); Shapin (1979)
10. The role of metaphysics in science has been emphasized by some of the great historians of science. See Koyre (1957); Koyre (1965); Burt (1954); Hall, A.R. (1963, 1966)
11. Hooykas, R (1972) presents a case for the role that religious beliefs played in the development of modern science. His position reveals the influence of both Weber and Merton who emphasized the significance of the role that Puritanism played in the rise of modern science.
12. Barnes (1977); Bloor (1976)
13. Koestler (1968)
Feuer (1963); (1971)
14. Polanyi (1964)
15. Such a distinction has been suggested by Laudan (1977) p.197
16. Collingwood (1940) p.48
17. Brown (1977) p.104
18. ibid. p.105
19. Gillispie (1960)
20. Bunge (1963)
21. A symposium on the issue of simplicity can be found in Phil. Sc. 28 (1961) p.109. Participants include R. S. Rudner, M. Bunge, N.

- Goodman, R. Ackerman, and S. F. Barker. Others who have dealt with this issue are Quine (1960) who makes it a central criterion in choosing conceptual networks. Hesse (1974) devotes a whole chapter to explicating the notion. Also refer Sober, E's Simplicity which is reviewed in Phil. Sc. (1976) p.412
22. Misner, Thorne and Wheeler (1973) pp.1050-1055 give a semi-historical account of Eotvos' experiments at the end of the last century which provided a test of the inverse square law of gravitation to an unprecedented degree of accuracy..
 23. Humphreys (1968) p.37
 24. Koslow in Stuewer (1970) p.356
 25. Hesse (1974) p.236
 26. ibid. p.234
 27. Mach (1898) pp.192-193
 28. The criteria (1) and (2) have been emphasized by Popper. For Popper a good scientific theory must not only be able to cover a larger domain of facts and, therefore, be more general, but it must also be more specific in its description of these facts. Such theories, he says, are highly falsifiable - i.e., they have more potential falsifiers. The third criterion plays an important rôle in Poincare's epistemology. Popper (1972) pp.13-21
 29. Suppe (1974) p.65
 30. Hesse (1974) p.237
 31. Lakatos (1970) p.118
 32. Popper (1972) p.16; Zahar (1973)
 33. Feyerabend (1974) pp.26-27
 34. Gillispie (1960)

Chapter 3

1. For an account of the development of logical empiricist views see Jorgensen (1953); for a comprehensive account of critical appraisal of holist views see Papineau (1979).
2. Carnap (1928) proposed that observation terms should be given a phenomenalistic (sense-data based) interpretation. In Carnap (1966) he is prepared to allow a physicalistic interpretation. In fact, he appears to view the two interpretations as equivalent in that they are alternative modes of describing the same thing.
Carnap (1963) pp.50-51
3. Hesse (1974) p.9
4. Hume's first volume of his Treatise opens with the statement "All the perceptions of the mind resolve themselves into two distinct kinds, which I shall call IMPRESSIONS and IDEAS." Hume (1967) p.1
5. Nagel (1961) Chapter 5
6. *ibid.* pp.90-94
7. *ibid.* p.83. Nevertheless, he claims that observation terms are associated with definite overt procedures that fix the meanings and ranges of application of those terms (*ibid.* p.89). However, he argues that the theoretical terms like electron, neutrino and gene cannot have their meaning fixed in this manner since "there are no overt procedures for applying those terms to experimentally identifiable instances of the terms" (*ibid.* p.85)
8. *ibid.* p.98
9. *ibid.* p.87

...although the theoretical terms are not assigned a unique set of determinate senses by the postulates of a theory, the permissible senses are limited by those satisfying the structure of interpretation into which the postulates place the terms.

Accordingly, when the fundamental postulates of a theory are altered, the meaning of its basic terms are also changed, even if (as often happens) the same linguistic expressions continue to be employed in the modified theory as in the original one."

On the other hand, observational terms will continue to retain their meaning in spite of theory changes and can be used to formulate experimental laws (as distinct from theories) that are (relatively) incorrigible. [ibid.p.86]

10. Wittgenstein (1961) Propositions 1.1; 4.21; 4.211
11. Carnap (1936-37) Reprinted in Feigl and Brodbeck (eds.) 1953
Section 8 pp.63-64
12. Achinstein (1968) Chap. 5 Sect. 4
13. Putnam (1962)
14. Hesse (1974) p.29
15. Such a view is close to the one proposed by Hesse and which, is itself, an elaboration of Quine's position. Thus Hesse argues that
(i) All descriptive predicates, including observational and theoretical predicates, must be introduced, learned, understood and used, either by means of direct empirical associations in some physical situations, or by means of other descriptive predicates which have already been so introduced, learned, understood and used, or by means of both together.
and (ii) No predicates, not even those of the observation - language, can function by means of empirical associations alone. Hesse (1974) p.11

Similarly, Suppe (1972) has suggested using subscripts to distinguish the observable and theoretical occurrences of properties. E.g., 'red' could be used to refer to the observable property red and 'red' the nonobservable property. These

suggestions are perfectly in accord with the view that I have proposed which acknowledges that all terms can have observational and theoretical meaning.

16. Churchland (1979). Arguing that perception is far more plastic than traditionally envisaged he suggests that

"in large measure we learn, from others, to perceive the world as everyone else perceives it. But if this is so, then we might have learned, and may yet learn, to conceive/perceive the world in ways other than those supplied by our present culture. After all our current conceptual framework is just the latest stage in the long evolutionary process that produced it..." p.7

In particular he recommends that

"... one's perceptual judgements be made, if possible, within the terms of the best available world-theory. An examination of the capacities of our own sensory system indicates that, with respect to modern physical theory, this is a live possibility..." p.37

17. Feyerabend (1962) pp.35-62. He argues that psychological processes cannot give terms meaning just as physical processes cannot give meaning to a physical fact like the pointer of a scientific instrument being at a certain point on a dial. An instrument reading acquires meaning only when interpreted in the context of a scientific theory - it is not a matter of responding to external inputs. Kuhn (1970a) pp.101-102 also raises a radical theoretical-context dependent notion of terminological meaning to argue his incommensurability thesis.
18. Koestler (1978) pp.33-34
19. This Quinean equivocation has been noted. See Hesse (1974) p.26
20. Quine (1951) in Harding (1976) p.60
21. Quine (1960) p.44. The point is repeated in Quine and Ullian (1970) p.16:-

"Observation sentences, after all, are the sentences for which the evidence is present whenever the sentences are truly affirmed. It

would strain the very meaning of the words, in such sentences, to suppose any appreciable fallibility; for the worlds are themselves acquired through the association of observation sentences with the observable circumstances of their utterance."

Nevertheless even here Quine is prepared to allow some trace of fallibility - but only a trace.

22. Hesse (1974) p.15
23. A number of critics, including Shapere (1966) and Scheffler (1967) have argued that Kuhn's and Feyerabend's views make relativism inescapable. This has led Kuhn to withdraw some of his radical claims [Kuhn (1970a) see postscript, and (1970b)], but Feyerabend's response has been to embrace explicit relativism [Feyerabend (1975)]
24. Landan (1977) p.143
Popper (1965) p.112 adopts a similar view
25. For a historical account of the development of Saccheri's geometry see Sklar (1974) pp.17-19
26. Feyerabend (1975) p.282
27. Radnitzsky (1973) pp.212-214
28. Feyerabend (1975) p.67-68
29. Feyerabend (1975) pp.159-160
Also Kuhn (1970a) argues a similar view, especially in Chap.X
30. Feyerabend (1975) p.284
31. Feyerabend (1975) ch.18
Kuhn (1970a) pp.160-161
32. Brown (1977) pp.132-134 describes dialectical logic as a content-logic rather than a formal logic. He suggests that though the latter allows formal reconstruction of completed research programs only the former can analyse (both the relations between successive

theories and the actual research process).

33. Feyerabend (1975) pp.39-40

34. Kuhn (1970a) p.77

Lakatos (1970) p.119

35. Suppe (1974) p.170 has described Feyerabend's philosophy of science as an attempt to develop the Popperian falsificationist account of science without presupposing that there is a neutral observation language.

Chapter 4

1. Duhem, P. (1962) The Aim and Structure of Physical Theory, especially Chapter VI. Reprinted in Harding (ed) (1976) pp1-40
2. Kuhn (1970a) especially chapter X.
Feyerabend (1965); (1970b); and Chapter 5 in (1975)
3. This assumption is in fact at the heart of the problem of reduction in the logical empiricist tradition. That a new theory, in limiting cases, approximates to an earlier one ensures that its scope includes that of its predecessor. Even antipositivists like Lakatos and Popper assume that this requirement is necessary for scientific progress. Lakatos (1970) p.118; Popper (1972) p.16.
4. This is a point emphasized by Feyerabend (1975) p.178. He argues that the Lorentz theory, e.g., was not replaced by a single successor but at least two - Einstein's theory of relativity and the quantum theory which divided its domain amongst themselves. Also see Feyerabend's note 'Zahar on Einstein' in BJPS March 1974. However, like the early empiricists Feyerabend assumes that this sort of replacement necessarily implies that rational evaluation of theories is impossible. This is still to presume that rationality requires that the scope of new theories be larger than earlier ones - an assumption that is unnecessary as we shall see. See p.155
Feyerabend (1975)
5. Neurath in Ayer (ed.) 1959 p.203
6. Hempel (1952) p.621
7. Lakatos (1970) p.94
8. Popper (1959) Section 85
9. Braithwaite (1953) pp.367-368

10. Duhem (1962) in Harding (ed.) (1976) p.8
11. Quine (1951) in Harding (ed.) (1976) p.60
12. Hanson (1958) (1969)
Toulmin (1953) (1961)
13. Kuhn (1970b) p.15
14. Kuhn (1970a) Chap. X
15. Kuhn (1970b) p.13
16. Feyerabend (1975)
17. ibid. p.67
18. Lakatos (1970) pp.129-130
19. Popper (1959) p.104
20. Feyerabend (1975) Chap. 12
Kuhn (1957)
21. Pickering (1897) pp.92-94
22. Kayser (1897)
23. Jammer (1966) p.82
24. ibid. pp.146-151
25. Berkson (1974) pp.240-241
26. Laudan (1977) p.21
27. Shapere (1974) p.527
28. Whittaker (1951) Vol. 1 p.175
29. Collingwood (1956) p.329. Regarding science specifically he says
"Progress in science would consist in the supercession of one theory by another which served both to explain all that the first theory explained, and also to explain ... 'phenomena' which the first ought to have explained but could not" p.332
30. Popper (1965) p.232
31. Lakatos (1970) p.118
32. Kuhn (1970) p.169

33. Feyerabend (1975) p.176
34. *ibid.* Chap. 12
35. *ibid.* p.153
36. *ibid.* p.151-152
37. *ibid.* p.153-154
38. Lakatos p.121 (1970)
Feyerabend (1970a) p.205
39. Newton's Queries
40. Hirosige, T. (1969)
41. Kuhn (1970a Chap. VIII) was one of the first philosophers to point to the significance of transition periods of crisis before a scientific revolution. Nothing in the traditional verificationist, confirmationist or falsificationist methodologies prior to his work required one to suppose that a transition period was inevitable in any conceptual realignment. The methodology I have proposed reveals that such periods are inevitable as the new theories first win admittance; then increase their relative explanatory indispensability with respect to their earlier rivals (thereby generating a period of crisis); win more converts as they become preferable; and finally achieve acceptance by becoming superlatively preferable to all their predecessors. The sequence Kuhn revealed in history - normal science, developing anomalies, crisis, resolution of crisis - is reflected in the way a new network of theories slowly appropriate the domain of their predecessors.
42. Landan offers a number of examples - Newton's optics failed to explain refraction in Iceland Spar which Huyghen's optics did; the

early 19th century caloric theory failed to explain heat convection and generation that Rumford's theory had solved in the 1790's; Dalton's atomic chemistry did not account for some phenomena that earlier theories of electric affinity managed to do; vorticular theories solved the problem of the mutual repulsion of negatively charged electric bodies which the new Franklin paradigm did not. Laudan (1977) p. 149.

Kuhn had made a similar point earlier. Kuhn (1970a) p.157

43. As Toulmin (1970) pp.43-44 has pointed out:

"... the professional careers of numerous physicists spanned the years from 1890 to 1930, and these men lived through the change from the Newtonian to the Einsteinian system of thought. If the complete breakdown in scientific communication which Kuhn treats as the essential characteristic of scientific revolution had in fact been manifested during this period, one should be able to document it from the experience of the men in question. What do we find? If the conceptual change involved in the transition was as deep as Kuhn claims, these physicists at any rate appeared curiously unaware of the fact."

Toulmin suggests that just as neither uniformitarianism nor catastrophism had been found adequate to deal with the problems of geology and palaeontology in the nineteenth century, and we had to recognize elements of continuity and change as evolutionary theory did, the same would have to be seen in scientific changes. This is, in fact, what we would see if we adopted the view that revolutions are neither cumulative nor disjunctive, but metamorphic.

Chapter 5: Paradigms and the Sociology of Knowledge

1. Kuhn (1970a) and Kuhn (1970b). Similar views have been proposed by Burt (1954) and Koyre (1957)
2. Masterman (1970) p.61-62
Shapere (1964) Reprinted in Gutting (1980) p.38
3. Kuhn (1970) pp.174-176
4. Gutting (1980)
5. Kuhn (1970a) p.44 He writes:-
"scientists can often agree that a Newton, Lavoisier, Maxwell or Einstein has produced an apparently permanent solution to a group of outstanding problems and still disagree, sometimes without being aware of it, about the particular abstract characteristics that make those solutions permanent. They can, that is, agree in their identification of a paradigm without agreeing on, or even attempting to produce a full interpretation or rationalisation of it."
6. *ibid.* pp.117-118
7. *ibid.* p.42
8. For an argument for the retention of the traditional distinction refer Feigl (1970). Reichenbach (1938) p.6 and p.382 was one of the first to emphasize this distinction.
9. Two important alternative viewpoints are those of Lakatos (1970) and Laudan (1977). Refer especially to Laudan's chapter "From Theories to Research Traditions". Both these positions will be discussed in greater detail a little later.
10. This is, in fact, one of the major reasons for the tremendous impact his ideas have had on the development of the sociology of knowledge. See Gutting (1980).
11. Kuhn (1970a) p.46
12. *ibid.* p.45
13. Masterman (1970) p61-65

14. Shapere (1964) reprinted in Gutting (1980) p.38
15. Laudan (1977) p.75
16. Maxwell made numerous unsuccessful attempts to develop a mechanical model of his electromagnetic theory. A good account of his endeavours can be found in Beakson (1974) pp.148-184
17. A brief historical treatment of the evolution of Saccheri's ideas is given Sklar (1974) pp.17-19
18. Popper (1970) pp.55-56
19. Feyerabend (1970a) p.198-199
19. Lakatos (1970) pp.121-122
Feyerabend (1970a) p.212
20. Feyerabend (1975) pp.29-33
21. Kuhn (1970a) pp.43-51; pp.187-191
22. Lakatos (1970) p.133
23. ibid, p.135
24. Suppe (1979) p.661 supports this view:-
"Research programmes are reminiscent of Kuhnian 'normal science', though the guiding principle of a research programme, Lakatos' positive and negative heuristics, are very unlike Kuhnian paradigms or disciplinary matrices."
25. Feyerabend (1970a) p.215
26. Lakatos appears to want history to be the judge. McMullin has criticised him for making such a requirement.

"For, if the history used in the test is reconstructed history, then the use of it to test a methodology is either question-begging or circular depending on whether or not a different or same methodology is used to effect the reconstruction. And if it is, actual, unreconstructed, history then Lakatos' research programmes methodology fails to pass the test just as surely as do other methodologies he considers; for Lakatos himself acknowledges that a number of historical episodes he examines do not proceed in accordance with his research programs methodology, although they could, and should have." McMullin (1970) pp.313-334.

27. Lakatos (1970) p.118 .
28. Laudan (1977) p.77
29. ibid. p.81
30. In short, for Laudan, "a research tradition is ... a set of ontological and methodological "do's" and "don'ts". ibid. p.80
31. ibid. p.89
32. ibid. p.85
33. Popper (1959) Sections 11 and 10
34. Watkins (1958)
35. Watson (1956) Reprinted in Watson "What is Behaviourism?" in Readings for an Introduction to Philosophy, (eds.) Hamilton, Reagan and Tilghman (1976) p.174
36. Skinner (1953) pp.30-31
37. Koestler (1967) p.17
38. Losee (1972) p.20. Actually Ptolemy's position was somewhat more complex, and he tended to equivocate on this issue between realist and instrumentalist views.
39. Kuhn (1957) p.169
40. Laudan (1977) p.85
41. Gillispie (1960) Chap. V "Science and the Enlightenment"
42. Burchfield (1975)
43. Bloor is one of the staunchest supporters of such a view. Bloor (1976) p.1. He writes:-

"Can the sociology of knowledge investigate and explain the very content and nature of scientific knowledge? Many sociologists believe that it cannot. They say that knowledge as such, as distinct from the circumstances surrounding its production, is beyond their grasp. They voluntarily limit the scope of their own enquiries."
44. Laudan (1977) p.198

45. Bloor (1976) p.9
46. ibid. p.26-28
47. ibid. Chapter 1. Bloor has delineated, and argued against, these three sorts of criticisms that have traditionally been levelled against any possibility of an externalist account of the content of accepted scientific theories. My approach, however, differs from his though I accept his classification.
48. Merton (1970) p.75 denies that the content of scientific theories can be explained in sociological terms. He says:

"specific discoveries and inventions belong to the internal history of science and are largely independent of factors other than the purley scientific."

Mannheim (1952) p.135 endorses this position. He concludes that the historical development of mathematics and the natural sciences are largely determined by "immanent factors".
49. Richter (1973) p.6
50. Bloor (1976) p.8
51. ibid. p.12
52. Laudan (1977) p.201
53. ibid. p.202
54. ibid. p.202
55. Lakatos argues for the primacy of internal history and seeks to make externalist accounts possess only peripheral value

"...internal history is primary, external history only secondary, since the most important problems of external history are defined by internal history. External history either provides non-rational explanation of the speed, locality, selectiveness etc. of historical events as interpreted in terms of internal history; or when history differs from its rational reconstruction, it provides an empirical explanation of why it differs. But the rational aspect of scientific growth is fully accounted for by one's logic of scientific discovery". Lakatos (19712) p.9

56. Bloor (1976) p.9 concedes the viability of a theory of internal rationality, albeit somewhat reluctantly. "In its own terms the teleological model is, according to him, perfectly consistent, and there are no logical reasons why anyone should prefer the causal approach to the goal directed view." However, we have seen that there is such a reason - if that someone intends to explain, rather than evaluate, the content of an accepted theory.

57. Grunwald in Mannheim (1952) p.29 says:

"it is impossible to make any meaningful statement about the existential determination of ideas without having any Archimedian point beyond all existential determination... No long argument is needed to show beyond doubt that this version of sociology, too, is a form of scepticism and therefore refutes itself. For the thesis that all thinking is existentially determined and cannot claim to be true claims itself to be true."

Similarly Bottmore (1956) p.52

"For if all propositions are existentially determined and no proposition is absolutely true, then this proposition itself, if true, is not absolutely true, but is existentially determined."

58. Laudan (1977) p.201. Mannheim confronts the same dilemma. He writes that:-

"once we have familiarized ourselves with the conception that the ideologies of our opponents are, after all, just the function of their position in the world, we cannot refrain from concluding that our own ideas, too, are functions of a social position."

However, somewhat, unconvincingly, he subsequently claims immunity for scientists like himself because they belong to "the relatively socially unattached intelligentsia." Mannheim (1952) p.145 and p.252ff. This dilemma, as we shall see, is a false one.

59. Bloor (1976) p.13-14

60. Forman (1971)

61. Bloor (1976) p.5 This is Bloor's fourth requirement for his strong programme for the sociology of knowledge. We have not considered

his other three requirements because they are important only for a particular type of sociological theory - namely Bloor's sociological causal theory. The reflexivity condition, however, must be satisfied by any sociological theory - causal or otherwise.

62. Reichenbach (1938) p.6; p.382
63. The notion of truth as idealised acceptable representation - and not as correspondence or coherence - is developed more fully in chapter 7 of this work.

Chapter 6

1. Laudan (1977) p.127 writes:

"...what I am suggesting is that we apparently do not have any way of knowing for sure (or even with some confidence) that science is true, or provable, or that it is getting closer to the truth. Such aims are utopian, in the literal sense that we can never know whether they are being achieved..."

Kuhn (1970a) pp.170 goes even further:-

"We may, to be precise, have to relinquish the notion, explicit or implicit, that changes of a paradigm carry scientists and those who learn from them closer and closer to the truth"

2. Wittgenstein (1961) 2.1 - 3.01. Thus proposition 3.01 reads "The totality of true thoughts is a picture of the world".

3. Popper (1972) p.317

Rescher (1973) p.124

4. Rescher (1973) pp.7-8

5. For a good critical discussion refer Armour (1969) Chapter 2

"Correspondence theories": Also Rescher (1973) p.8 lists the major difficulties connected with all correspondence theories.

6. Popper has argued, rightfully, that not even direct observation reports involve universals:-

"Every description uses universal names (or symbols, or ideas); every statement has the character of a theory, of a hypothesis."

This follows from the theory-ladenness of experience thesis.

Popper (1959) pp.94-95

7. Armour (1969) argues that "the correspondence theory necessarily requires that every true proposition should correspond to something - and, therefore, the world becomes very rich in peculiar entities." p.49. Such criticisms and others I discuss apply only to the criterial variant of the correspondence theory. They are not a critique of the correspondence theory of truth if this is

interpreted in Tarski's sense of the semantic theory.

8. Popper (1972) p.311
9. Schlick "The Foundation of Knowledge" in Ayer (1965)
10. Neurath "Protocol Sentences" in Ayer (1965) p.201 and p.203
11. White (1970) p.111
12. Schlick in Ayer (1965) p.215
13. Thus Schlick writes:-

"If science is taken to be a system of statements in which one's interest as a logician is confined to their logical connection, the question of its basis... can be answered arbitrarily. For one is free to define the basis as one wishes. In an abstract system of statements there is no priority and no posteriority ... If attention is directed upon the relation of science to reality... the problem of the 'basis' changes then automatically into that of the unshakeable point of contact between knowledge and reality. We have come to know these absolutely fixed point of contact (viz. the protocol statements giving observation reports), the confirmation, in their individuality: they are the only synthetic statements that are not hypotheses" pp.226-227 in Ayer (1959)

14. Quine (1960) p.44
15. Rescher (1973) pp.53-54
16. ibid. p.54
17. Hesse (1974) p.54
18. This excessive holism is reflected in the way the coherence theory cannot separate statements that are reports of experience from other statements. Neither is it possible; within the framework of the theory as offering the meaning of truth, to effect without artificial processes, such a separation. Armour (1969) p.116
19. Putnam (1981) p.55 also defines truth as an epistemic notion. However, the definition I have proposed is offered through a definite and precisely given theory of scientific rationality. This notion is related to Dewey's pragmatic conception of truth as warranted assertibility, but has none of the pragmatic criterion's

unfortunate repercussions.

20. James (1948) p.156 in "What Pragmatism Means" writes:-

"in this world, just as certain foods are not only agreeable to our taste, but good for our teeth, our stomach and our tissues; so certain ideas are not only agreeable to think about, or agreeable as supporting other ideas we are fond of, but they are also helpful in life's practical struggles."

However, this does not get around the usual objection to the pragmatic theory that sometimes what is agreeable to our taste may not be good for our teeth. How do we make decisions in such cases? Pragmatic criteria are equally open to deciding either for good taste or for good teeth without telling us which is to be given priority, or how they are to be balanced.

21. Russell (1910)

Lovejoy (1908)

22. Armour (1969) p.144

23. This reveals a need for a programme that would guide scientists towards the creation of theories which would simultaneously aim at being both adequate in representing the world (i.e., be true), and be adequate in representing human interests (i.e., be ethically, aesthetically or socially valuable). Such a programme has been proposed by Leach in "The Dual Function of Rationality":

"One of the central issues is whether the historian of science merely positively describes and explains the course of past theorizing or whether in so doing he also prescribes certain methodological norms of scientific enquiry. And if he manages the later, how are his dual positive and normative functions related? ...the historian, as any social scientist, must invoke a rational account of scientific developments, both intellectually and socially, both internally and externally. But to do this he needs a general theory of action with its core rationality theory. One might in fact, want to distinguish between internal and external

history, on this basis, as the difference between invoking purely epistemic utilities or including pragmatic utilities as well."

The importance of a unification programme of this sort should not be overlooked, especially in relation to constructing a theory to account for the content of accepted scientific theories. Such a global programme is outside the scope of my discussion which is concerned only with epistemic rationality and revealing the possibility of a theory of external rationality.

24. Goodman (1978) p.4

25. *ibid.* p.4

26. *ibid.* p.6

27. *ibid.* p.94

28. *ibid.* p.22

29. Putnam (1981) p.62

30. *ibid.* p.73

31. *ibid.* p.52

32. *ibid.* p.49

33. *ibid.* p.xi

34. *ibid.* p.xii

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